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PROCEEDINGS

OF THE



ROYAL SOCIETY OF LONDON.

From November 21, 1872, to November 27, 1873.

VOL. XXI.

LONDON:
PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

MDCCCLXXIII.

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ERRATA.

Page 214, line 5 from bottom, *for* accessible *read* sensible.

„ 216, „ 22 „ top, *for* opposite *read* consequent.

„ „ „ 26 „ „ *for* Whenever *read* However.

„ „ „ 27 „ „ *for* is *read* be.

„ 243, „ 1 „ „ *for* rod-like *read* rod-shaped.

„ 244, „ 22 „ „ *for* corroborated *read* corroborative.

„ 301, „ 12 „ „ *for* Blarifone *read* Blanford.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 21, 1872.

Sir JAMES PAGET, Bart., D.C.L., Vice-President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Sir George Biddell Airy, K.C.B., M.A., D.C.L., LL.D. (Astronomer Royal).

Treasurer.—William Spottiswoode, M.A.

Secretaries.— { Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.
 { Prof. Thomas Henry Huxley, LL.D.

Foreign Secretary.—Prof. William Hallowes Miller, M.A., LL.D.

Other Members of the Council.—George James Allman, M.D.; Sir B. C. Brodie, Bart, M.A., D.C.L.; George Busk, F.R.C.S.; Prof. Robert Bellamy Clifton, M.A.; James Fergusson, D.C.L.; Thomas Archer Hirst, Ph.D.; J. Dalton Hooker, C.B., D.C.L., LL.D.; Joseph Prestwich, F.G.S.; Rear-Admiral G. H. Richards, C.B.; Prof. H. Enfield Roscoe, B.A., Ph.D.; Philip Lutley Sclater, M.A.; William Sharpey, M.D., LL.D.; Francis Sibson, M.D.; Major-Gen. R. Strachey, R.E., C.S.I.; Isaac Todhunter, M.A.; Sir Charles Wheatstone, D.C.L.

Mr. Abel, Mr. J. Evans, Mr. Hanbury, Mr. Mallet, and Mr. Scott having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Rev. Thomas Hincks and Prof. T. Rupert Jones were admitted into the Society.

The Presents received were laid on the Table, and thanks ordered for them.

The following communications were read:—

I. "Further Experiments on the more important Physiological Changes induced in the Human Economy by change of Climate." By ALEXANDER RATTRAY, M.D. (Edinb.), Surgeon R.N., H.M.S. 'Britannia.' Communicated by G. BUSK, F.R.S. Received July 1, 1872.

The following observations were made in 1871, during a passage from England to the Cape of Good Hope and back, to verify and extend those published in the 'Proceedings' of this Society for 1869-70-71.

I. *The Effect of Tropical Climate on the Respiration.*

TABLE I. To show:—*First*, that in four healthy adults the spirometric measurement of the lungs gradually rose and fell four times successively in migrating between tropical and temperate climates, and we may therefore conclude that it invariably does so; *second*, that the second rise and fall are the greater, partly owing to an increased range of temperature (3° F.), but chiefly to the freer respiratory action of the skin*.

No.			Near England. Lat. 50° N. March 28. Temp. 57° F.		Near Equator. Lat. 0°. May 2. Temp. 80° F.		Near Cape of Good Hope. Lat. 33° S. June 28. Temp. 56° F.		Near Equator. Lat. 0°. Aug. 29. Temp. 82° F.		Near England. Lat. 51° N. Nov. 2. Temp. 55° F.	
			Chest.	Spirom.	Chest.	Spirom.	Chest.	Spirom.	Chest.	Spirom.	Chest.	Spirom.
	Age.		in.	cub. in.	in.	cub. in.	in.	cub. in.	in.	cub. in.	in.	cub. in.
1.	40	{ height 5 ft. 8 in. weight 146 lbs. }	34½	252	34½	275	34½	248	34½	276	34½	246
2.	31	{ height 5 ft. 9½ in. weight 166 lbs. }	37½	277	37½	303	37½	273	37½	304	37½	273
3.	30	{ height 5 ft. 6 in. weight 154 lbs. }	37½	284	37½	300	37½	283	38½	306	39	276
4.	22	{ height 5 ft. 5½ in. weight 144 lbs. }	35	216	35	229	35	219	35½	234	36	213
	Average	{ capacity	257	277	256	280	252
		{ rise or fall	20 rise.	21 fall.	24 rise.	28 fall.

The tropico-temperate spirometric range varied in these from 10 to 31, average 23 cubic inches; i. e. from 4½ to 11, average 8·7 per cent. of the spirometric measurement of the lungs. The range might have been greater had the thermometric range been so; but it doubtless has its healthy physiological limits. The tropico-frigid spirometric range is probably not much higher, as the bodily temperature in cold regions is usually maintained by food, fires, &c. at about the same standard as in the temperate zone. This altered spirometric measurement is permanent, i. e. lasts as long as the residence in the new zone.

* This and subsequent facts appear to warrant a conclusion formerly expressed, viz. that one effect of frequent change of climate is to increase, within certain bounds, the vicarious capabilities of excreting organs.

Table II. shows :—*First.* That this law prevails between the ages of 14½ and 62, and therefore doubtless at all ages.

Second. That the rise and fall are greatest at the prime, and decrease towards either extreme of life.

Third. That at all ages, and for the same reason, the rise and fall are greatest in those of largest frame and most full-blooded.

Fourth. That for the same reason this rise and fall are greater, *ceteris paribus*, above than under puberty (marked *), and subsequently increase with the growth of the frame.

Fifth. That the normal expansion of the lungs and chest from growth only lessens slightly the spirometric reduction under cold, which often falls below its original standard.

Sixth. That after mid age the rise and fall, like the spirometric measurement itself, diminish with the progressive decay of the frame (cases 68 to 73).

Seventh. That in extreme old age (case 74) the shrivelling and less vascular lungs, skin, body, diminishing chest (cases 68, 71), respiratory and circulatory functions lessen and finally counterbalance the law.

Eighth. That obesity, i. e. an increased bulk of body, does not interfere with the last-mentioned effect of advancing age (case 69).

Ninth. That the rise and fall are not materially interfered with by an ordinary catarrh (case 55) or asthma (case 56).

The only exceptions to the law among the 47 cadets were (case 16) a suspicious chest, which gave no primary increase under heat; and case 41, which, instead of decreasing under cold, increased by 4 cubic inches by unusually rapid chest expansion from growth.

TABLE II. Climatic Spirometric range at different ages.

No.	Age.	South Temperate Zone. Lat. 35° S.; Temp. 56°-66° F. July 16, 1871.				Tropics. Near Equator; Temp. 82° F. Aug. 31, 1871.				North Temperate Zone. Lat. 50° N.; Temp. 53°-56° F. Nov. 2, 1871.			
		Weight.		Height.		Chest.		Spirom.		Weight.		Height.	
		lbs.	ft. in.	in.	cub. in.	lbs.	ft. in.	in.	cub. in.	lbs.	ft. in.	in.	cub. in.
1*	14.6	88	4 4	28½	111	89	4 7½	28½	115	95	4 8½	29	111
2*	14.8	118	5 3½	32½	186	119	5 4	33	202	120	5 4½	33	195
3*	14.11	99	5 2	30½	147	100	5 2½	30½	156	101	5 2½	30½	141
4*	15.1	97	5 2½	30	171	100	5 2½	30½	183	101	5 3	30½	163
5*	15.3	115	5 4	33	180	116	5 4½	34	195	115	5 4½	34½	186
6*	15.4	119	5 5½	32½	189	116	5 6	33½	201	121	5 6½	33½	192
7*	15.4	98	5 4½	30	174	98	5 4½	30	186	95	5 4½	30	165
8.	15.6	122	5 5½	32	199	124	5 6	32½	207	125	5 6½	32½	198
9*	15.6	121	5 4	31½	141	109	5 3½	31½	165	118	5 4½	31½	156
10*	15.6	96	4 11	30½	141	94	4 11½	30½	153	97	4 11½	30½	141
11*	15.6	100	5 3½	30½	158	95	5 4	30½	168	98	5 4	30½	162
12*	15.6	122	5 5½	33½	186	120	5 6	33½	201	119	5 6	33½	195
13*	15.6	99	4 9½	30	186	88	4 10½	30½	165	92	4 10	31½	162
14.	15.7	153	5 9	36½	237	150	5 8½	36½	261	158	5 9½	37	256
15.	15.7	122	5 8½	32½	213	118	5 9	33½	240	118	5 9	34	225
16*	15.7	105	5 4	28½	162	100	5 3½	28½	162	106	5 3½	29½	159
17.	15.7	141	5 7	35½	226	138	5 7	35½	237	137	5 7	35½	220
18*	15.8	110	5 4	30½	168	105	5 4	31½	175	106	5 4½	31½	165

TABLE II. (continued).

No.	Age.	South Temperate Zone. Lat. 35° S.; Temp. 55°-65° F. July 16, 1871.				Tropics. Near Equator; Temp. 82° F. Aug. 31, 1871.				North Temperate Zone. Lat. 50° N.; Temp. 52°-56° F. Nov. 2, 1871.			
		Wei ht.	Height.	Chest.	Spirom.	Weight.	Height.	Chest.	Spirom.	Weight.	Height.	Chest.	Spirom.
		lbs.	ft. in.	in.	cub. in.	lbs.	ft. in.	in.	cub. in.	lbs.	ft. in.	in.	cub. in.
19.	15'8	150	5 9½	36	216	149	5 9½	36½	234	156	5 9½	36½	204
20.	15'8	139	5 9½	35½	228	141	5 9½	35½	256	144	5 10	35½	243
21.	15'8	123	5 4	35½	216	126	5 4½	35½	231	127	5 4½	35½	213
22*.	15'8	121	5 6½	34½	219	120	5 7	34½	231	121	5 7	34½	225
23*.	15'9	132	5 5½	35½	228	132	5 5½	35½	240	130	5 5½	35½	216
24.	15'9	136	5 5½	36½	216	137	5 6½	36½	237	138	5 6½	37	231
25.	15'9	161	5 11	36½	252	160	5 11	36½	264	160	5 11	36½	246
26*.	15'9	115	5 5½	33½	189	114	5 5½	33½	207	119	5 5½	33½	198
27*.	15'9	131	5 8½	33	192	125	5 8½	33	198	127	5 9½	33½	192
28.	15'9	137	5 3½	35½	222	141	5 3½	36½	240	133	5 3½	36½	222
29*.	15'9	109	5 3	32½	168	108	5 2½	33	197	112	5 3½	33½	186
30.	15'10	125	5 6½	32	183	124	5 6½	33½	189	123	5 6½	33½	180
31.	15'10	110	5 5	32½	192	109	5 5½	33	204	114	5 5½	33½	186
32.	15'11	138	5 7½	36½	219	136	5 7½	36½	237	135	5 8	36½	210
33.	16'0	141	5 9½	35½	237	137	5 9½	35½	248	139	5 9½	35½	225
34.	16'0	146	5 10½	36	252	144	5 10½	36½	267	147	5 11	36½	258
35*.	16'1	116	5 4½	33½	186	112	5 4½	34½	204	114	5 5½	34½	192
36.	16'1	140	5 8	35½	207	142	5 8	36	219	141	5 8	36½	198
37.	16'2	137	5 2½	35½	213	135	5 2½	35½	225	131	5 2½	36	219
38*.	16'2	111	5 1	34	189	108	5 1½	34	208	114	5 2	34½	201
39*.	16'2	113	5 7½	32	189	111	5 7½	32½	195	113	5 7½	32½	180
40.	16'2	131	5 8½	34½	228	126	5 8	34½	240	129	5 9	35	234
41*.	16'3	102	5 3½	30½	150	99	5 4	30½	156	99	5 4½	31	160
42*.	16'3	103	5 2½	29½	147	107	5 2½	29½	153	114	5 3½	30	144
43.	16'3	152	5 10½	35½	247	150	5 10½	36	252	156	5 10½	36½	231
44*.	16'5	112	5 2½	34	165	114	5 3	34½	180	119	5 3	34½	171
45.	16'5	136	5 5½	35½	205	136	5 5½	35½	212	131	5 6½	35½	192
46*.	16'6	107	5 3½	31½	172	105	5 4	32	183	109	5 4	32½	168
47.	16'8	130	5 7½	32½	196	127	5 7½	32½	228	128	5 7½	32½	207
48.	17'5	150	5 7	36½	213	145	5 7	36½	228	149	5 7½	36½	219
49.	18'2	148	5 4½	36	222	143	5 4½	36½	234	143	5 4½	36½	219
50.	18'8	149	5 8	35½	261	145	5 8	35½	267	146	5 8½	35½	249
51.	19'7	164	5 8	38½	264	162	5 9	38½	282	158	5 9	38½	264
52.	20'1	158	5 11½	37½	258	157	5 11½	37½	267	158	5 11½	37½	249
53.	20'6	142	5 6	34½	216	137	5 6	34½	234	141	5 6	35½	198
54.	20'6	150	5 10½	35½	264	143	5 11	36	300	145	5 11	36½	288
55.	21'5	161	5 8	35½	210	156	5 8	35½	225	160	5 8	35½	213
56.	21'9	172	5 9½	38	243	172	5 9½	38	246	174	5 9½	38	240
57.	22'2	152	5 10	35	256	148	5 10	36	264	149	5 10	36	236
58.	22'2	152	5 7½	36½	228	152	5 7½	36½	243	156	5 7½	36½	222
59.	22'2	144	5 5½	35	219	143	5 6	35½	234	144	5 6½	36	213
60.	23'2	139	5 6½	34½	201	130	5 6½	34½	228	135	5 6½	34½	207
61.	30'0	154	5 6	37½	283	154	5 6	38½	306	156	5 6	39½	276
62.	31'0	166	5 9½	37½	273	166	5 9½	37½	304	164	5 9½	37½	273
63.	40'0	146	5 8½	34½	248	148	5 8½	34½	276	145	5 8½	34½	246
64.	40'0	208	198
65.	41'0	196	186
66.	42'0	247	222
67.	43'0	207	204
68.	45'0	145	5 7	38½	234	141	5 7	38½	239	140	5 7	38	231
69.	45'0	168	5 8½	38½	237	162	5 8½	38½	246	160	5 8½	38½	234
70.	46'0	249	226
71.	47'0	152	5 8½	39	270	152	5 8½	38½	273	154	5 8½	38½	270
72.	47'0	152	5 9½	36½	216	148	5 9½	36½	224	149	5 9½	36½	213
73.	48'0	163	5 10	38½	186	164	5 10	38½	195	161	5 10	38½	177
74.	62'0	140	5 7½	35½	210	139	5 7½	35½	201	138	5 7½	35½	195

TABLE III. shows that, as with latitude, change of altitude, i. e. temperature, causes a rise and fall in the spirometric measurement.

Place &c.			Spirometric measurement of lungs.
			cubic inches.
A. R., et. 40. Ascension. Lat. 8° S.	Aug. 14. Level of Sea.	Temp. 75° F. ...	266
	Aug. 14. Green Mountain (2000 feet high).	Temp. 67° F. ...	249
	Aug. 15. „	Temp. 65° F. ...	243

This gives a difference of 18 cubic inches from a 10° F. reduction of temperature. Summer heat and winter cold in temperate latitudes, and also the atmosphere of artificially heated or cooled rooms or wards, have corresponding results.

That this spirometric law of climate has little except perhaps a secondary and minor connexion with an increase or decrease in the capacity of the bony chest is shown :—

1st. By the usual absence of any change in the circumference of the adult chest when the rise and fall are greatest (Table I., cases 1 and 2).

2nd. By the slight interference of growth of chest with the manifestations of the law, both in the adult (Table I., cases 3 and 4), and especially the cadets (Table II., cases 1 to 47) and junior officers (Table II., cases 48 to 60).

The spirometric measurement of the lungs is probably occasionally increased by actual chest expansion when an increased respiratory function is suddenly required in health or disease. But this is exceptional; and in emergencies, in cold and warm climates alike, this result is usually effected in a more natural and easier way, viz. by increasing the frequency of the respiration.

The only theory that will satisfactorily explain this tropical increase in the spirometric measurement of the lungs, with its peculiarities, and correlate it with other apparently opposed respiratory phenomena, such as slower and gentler inspiration, and also the *post mortem* lightness of the lungs observed by Francis and Parkes, is that it arises not from enlargement of the chest, but merely from a change in the relative quantity of blood and air in the lungs. The law of the lungs is merely a sequel of the greater law of the circulation, by which the entire blood-current is redistributed, and either attracted surfaceward by heat or driven inward by cold. The lungs, unaltered in size, contain less blood, actually and relatively, and more air in the tropics; they are less vascular, but more inflated. The oscillation in the relative proportion of blood and air follows an inverse ratio, the one rising, for an obvious reason, as the other falls. It is the rise or fall in the air, and not in the blood, which corresponds with the rise and fall of temperature. Blood is the dis-

placing, and air the displaced agent. It is in the blood that the fluctuation originates: when heat attracts this surfacewards, the amount drawn from the lungs is replaced by an equivalent bulk of air; under cold, which repels the blood inward, the air is again displaced by an equivalent of blood*.

Contrary to previous conviction, based on limited experiment, this variation in the spirometric measurement of the lungs is no index of the effect of change of climate on the *ordinary breathing*. Table III. shows that the latter, instead of deepening by heat like the former, does the reverse, and becomes *gentler*. As part of the same general physiological process, reflection points to this as the more likely sequel, and reconciles these apparently conflicting though allied phenomena.

TABLE IV.†

	South temperate zone. Lat. 35°. Temp. 54° F.	Tropics. Near Equator. Temp. 82° F.
A. R., set. 40. Average of 12 ordinary respirations. 3 P.M. }	cubic inches. 14.54	cubic inches. 13.25

Thus my respirations averaged $14\frac{1}{2}$ cubic inches in 54° F., and only $13\frac{1}{4}$ cubic inches in 82° F., i. e. less than 1.29 cubic inch. So that even if the respirations remained at $15\frac{1}{2}$ per minute, this alone would reduce the air used by 28,792 cubic inches, = 16.66 cubic feet, per day.

TABLE V. confirms former observation that respiration is *slower* in the tropics.

	Lat. 33° S. Temp. 56° F.	Equator. Temp. 82° F.	Lat. 53° N. Temp. 54° F.
Average of 15 { Rattray, set. 40 minutes at { Downer, set. 31 3 P.M. { Willcox, set. 20 Averages.....	No. 16 17 19 17.3	No. 15.6 16.6 16.2 16.1	No. 16.5 18.5 17.5 17.5

* This derivative action of heated air on the lungs corresponds to that of other derivatives, e. g. a hot bath increases their spirometric measurement by several cubic inches; so does a sordid dose of Dover's powder. Ordinary slow and prolonged exercise does the same by drawing blood to the functionally active muscles; and under violent exercise and a perspiring skin it rises from 10 to 15 cubic inches, even when the second result is taken in the evening, when the reduced temperature should have lessened it in accordance with the law of climate. This effect of exercise, however, is not so marked in the tropics as in temperate regions.

† This experiment was made by means of an easily inflated bladder, from which the air was measured by a graduated glass cylinder.

Thus in three adults it fell from an average of 17·3 per minute in 56° F. to 16·1 in 82° F., and again rose to 17·5 in 53° F. To close observation it was perceptibly slower and gentler*. However, as in temperate so in warm regions, the frequency of the respiration varies with the activity of the skin, purity of the blood, and need of the tissues for oxygen. Thus my tropical respirations ranged from 11 to 15, average 12·74 per minute; the range for the temperate zone being 13½ to 18, average 15·68.

These three well-marked tropical phenomena, viz. diminished lung vascularity, slower respiration, and gentler breathing, are closely related, and together indicate reduced lung work, the reverse for the temperate zone marking an increased function—thus proving that this varies with temperature, as with age, sex, height, &c. Their aggregate results vary with individuals, and perhaps races; but the following is a fair average of the reduction in the air respired and carbon excreted:—

TABLE VI.

		Cubic inches of air per respi- ration from Table IV.	Number of re- spirations per minute, from Table V.	Cubic inches of air respired per minute.
A. R.,	Tropics, temp. 82° F.	13·25	15·6	195·69 †
est. 40.	Temp. zone, temp. 54° F....	14·54	16·5	239·91

Difference per minute in favour of a temperate climate ... 44·22 cubic inches.

„ hour „ 2653·20 „

„ day „ 63676·80 „

18·43 per cent. = 36·85 cubic feet ‡.

Thus 239 cubic inches of air were respired per minute in the temperate zone, against 195 cubic inches in the tropics, = a difference of 44 cubic inches per minute, or 36·85 cubic feet per day, = 18·43 per cent.

Then, taking 10 oz. as the average amount of carbon thrown off by the lungs in the temperate climate of England §, this reduction of 18·43 per cent. gives 8·157 oz. as the amount for the tropics. The pulmonary excretion of watery vapour is doubtless correspondingly diminished.

* The experiments further proved that, like the circulation (pulse), the respiration increases towards infancy. Is the latter function, though actually less, relatively greater than from the rapid renewal and deoxidation of tissue and food?

† Corrected for heat in accordance with Dalton and Gay Lussac's law, the difference here being 27° F.

‡ Former calculation (Proceedings, June 16, 1870) made this as 27·81 cubic inches less of air per minute, = 10·9 per cent. = minus 1·1028 oz. of carbon per day. But this was based on limited data, in which the respiration was inaccurately fixed, and its depth calculated not from direct observation, but from the increased spirometric measurement of the lungs.

§ Mean of three estimates by Lavoisier and Seguin, Davy, and Allen and Pepys.

But this and the 1·843 oz. less carbon are perhaps partly excreted by the vicariously active skin and liver; but how far has yet to be determined. But it must be remembered that less carbon has usually to be eliminated in the tropics, both from the reduced and often altered ingesta, and lessened brain and body-tissue waste.

II. The Effect of Tropical Climate on the Bodily Development.

The following experiments prove that tropical climate, *per se*, interferes with the normal growth of the body. During the voyage forty-eight cadets, of the impressionable age of 14½ to 17, were weighed and measured in height six times at intervals of sixty days, *i.e.* after four successive changes of climate, their diet being abundant, fresh, and healthy. If lads stop growing, and still more if they lose flesh, it proves that some unhealthy influence is at work.

TABLE VII. shows:—*First*, that tropical heat interferes much with the bodily growth and development, as indicated by the *weight*, retarding it in most, and stopping it in some; *second*, that temperate climates have an opposite influence; *third*, that a second exposure to heat increases its wasting effect, and also lessens the subsequent building-up results of cold.

Exp.		Gain in weight.		Stationary in weight.		Loss in weight.	
		Per cent.	Average gain. lbs.	Per cent.	lbs.	Per cent.	Average loss. lbs.
1.	Effect of <i>heat</i> : 60 days in the tropics, temp. 70°–82° F....	56·26	3·81	14·58	...	29·16	3·07
2.	Effect of <i>cold</i> : 60 days in the S. Temp. Zone, temp. 55°–70° F.	93·75	6·35	6·25			
3.	Effect of <i>heat</i> : 60 days in the tropics, temp. 70°–82° F.	2·17	2·0	2·17	...	95·66	4·61
4.	Effect of <i>cold</i> : 60 days in the N. Temp. Zone, temp. 52°–70°	86·96	4·0	6·52	...	6·52	2·0
5.	Effect of <i>cold</i> : 60 days in the N. Temp. Zone, temp. 32°–50°	93·48	5·91	6·52			

The effect of climate on growth in height is somewhat different. As a rule this should be regular, though oftener, so to speak, *per saltum*, than with weight.

TABLE VIII. shows:—*First*, that tropical are more favourable to growth in height than temperate climates; *second*, that the influence of both is more decided on a second than a first exposure.

Exp.		Gain in height.		Stationary in height.	
		Per cent.	Average gain. inches.	Per cent.	Average inches.
1.	Effect of heat: 60 days in the tropics, temp. 70°–82° F.	68·75	0·64	31·25	
2.	Effect of cold: 60 days in the S. Temp. Zone, temp. 55°–70° F.	46·81	0·5	53·19	
3.	Effect of heat: 60 days in the tropics, temp. 70°–82° F.	69·57	0·52	30·43	
4.	Effect of cold: 60 days in the N. Temp. Zone, temp. 52°–70° F.	41·31	0·29	58·69	
5.	Effect of cold: 60 days in the N. Temp. Zone, temp. 32°–50° F.	23·91	0·43	76·09	

As heat thus makes the young shoot rapidly, it might be deemed favourable to the bodily development; but as the results are so different from those in Table VII., they cannot be taken as a true index of the healthiness of tropical climates, and must be studied with those of weight.

An increasing weight, with either an increasing (Table IX. col. 1) or even stationary height (col. 2), is a healthy sign; but if youths who should be growing in weight and height remain stationary (col. 3), or remain of the same height but lose weight (col. 4), or even grow in height while weight does not increase (col. 5), and still more if height increases while weight loses (col. 6), their physique must be deteriorating.

TABLE IX., with its two columns of averages contrasting these favourable and unfavourable signs, gives results similar to Table VII.; and shows:—*first*, that tropic heat is unfavourable to the bodily development; *second*, that cold climates are favourable to it; *third*, that the impairing effect of a second is greater than that of a first exposure to heat; *fourth*, that each successive exposure to heat lessens still further the bracing influence of cold.

Exp.		1.	2.	Per cent.	3.	4.	5.	6.	Per cent.
		H. + W. +	H. 0 W. +		H. 0 W. 0	H. 0 W. -	H. + W. -	H. + W. -	
1.	Effect of heat: 60 days in the tropics, temp. 70°–82° F.	19	7	54·1	2	7	6	7	45·8
2.	Effect of cold: 60 days in the S. Temp. Zone, temp. 55°–70° F. ...	21	24	95·7	1	0	1	0	4·2
3.	Effect of heat: 60 days in the tropics, temp. 70°–82° F.	1	0	2·1	0	13	1	31	97·8
4.	Effect of cold: 60 days in the N. Temp. Zone, temp. 52°–70° F.	15	25	86·9	1	1	2	2	13·0
5.	Effect of cold: 60 days in the N. Temp. Zone, temp. 32°–50° F.	10	33	93·4	2	0	0	1	6·52

Both the number who grew and the amount of increase were thus least in the tropics. These detrimental fluctuations necessarily affect both body and brain. If, therefore, we would produce strong, healthy, long-lived officers and seamen, fit for any work or climate, we should not send them unnecessarily to warm regions while as yet undeveloped lads.

The following are the chief results and inferences of these experiments:—

- 1st. The primary effect of great changes of climate is on the circulation, the blood being drawn surfaceward by heat and driven inward by cold.
- 2nd. The secondary and local results are an increase or decrease in the vascularity and function of the various tissues and organs of the entire body—the external increasing under heat, internal under cold, and *vice versa*.
- 3rd. Hence in the tropics the vascularity of the adult lungs is reduced by an average of 12 or 13 fluid oz.; their spirometric measurement by an average of 23 cubic inches; their function by 18·43 per cent., *i. e.* the use of 36·85 cubic feet less of air daily, excretion of 1·84 oz. less carbon, and 6·57 per cent. less of watery vapour.
- 4th. Hence, also, the nephritic vascularity and secretion are reduced in the tropics by 17½ per cent., and those of the skin increased by 24 per cent.
- 5th. In unison with the respiration the circulation is more languid in the tropics, as indicated by the reduced (several beats) and perhaps less forcible pulse.
- 6th. Notwithstanding the reduced circulatory and respiratory functions, and increased cutaneous exhalation, the temperature of the surface, and perhaps that of the blood and body generally, rises about 2° F. under tropical heat.
- 7th. The organs and functions of animal, like those of vegetable, life are affected by great changes of climate,—heat impairing the weight, strength, and health, *i. e.* the physique, at all ages and retarding growth in youth; while cold has an opposite effect.
- 8th. For obvious reasons these results vary with sex, temperament, size, &c.; and are most evident in the adult, decreasing towards either extreme of life.
- 9th. Collectively these changes are salutary, and meant to assimilate the various organs, functions, and body of the inhabitants of one zone to those of native races, to meet the requirements of the foreign climate, for which they are not, and perhaps never can be altogether, physiologically fitted.

II. "On the Mechanical Conditions of the Respiratory Movements in Man." By ARTHUR RANSOME. Communicated by Dr. SANDERSON, F.R.S. Received June 22, 1872.

(Abstract.)

The importance of an accurate measurement of the respiratory movements has been recognized by numerous physicians, and many different methods have already been in use for this purpose; these methods, however, have hitherto only given either simply the gross enlargement of the circumference of the chest, or the resultants of the motions of the different points on the chest-wall. Now, owing to the shape and mode of articulation of the ribs, the movements of any point on either side of the sternum may take place in three planes at right angles to one another, and need each to be recorded separately; and, owing to the variations in healthy breathing, it is further important that all these movements should be measured during one act of breathing.

The stethometer used in the following inquiry accomplished all these tasks, and measured simultaneously the three dimensions of movement of any point on the chest-wall. Graphic representations of these dimensions could also be obtained.

It was necessary to observe forced breathing chiefly; and it was noticed that when the button of the instrument was applied either to the middle of the sternum or on the end of one of the sternal ribs, the chief motions were forward and upward; that the forward motion was most equable, and started much more rapidly at first than the upward movement.

The earliest portion of the expansive act seems to be accomplished by an increase of the ordinary action of the diaphragm; then the chest gradually swells outwards and forwards, the ribs are raised, commencing usually at the lower ribs, the costal cartilages are straightened out, the head and shoulders are fixed, and the spinal column is curved backwards. In expiration after this effort the operation is reversed: variations in this action may be brought about by disturbing emotions, habit, suggestions from others, or antecedent ideas. Possibly, in consequence of this fact, the above account differs from that given by Haller, Sabatier, Majendie, Hutchinson, Sibson, and others. Care was therefore taken to ascertain its correctness, and it was tested,—1, by fastening tapes of paper round the chest, and noticing the order in which they were torn on deep inspiration; 2, by simultaneous tracings of the action of the 2nd and 5th or 6th ribs made, by means of Dr. Burdon Sanderson's stetho-cardiograph, on both males and females. The curves so produced were then carefully examined, and conclusions drawn from them as to the differences between male and female respiratory movements. In the Tables giving the dimensions of the motions of different points on the chest in healthy adult males, amongst other facts certain cases were noticed in which the parallelism of the movements of the sternum was interfered

with. Traube's observation as to the influence of the diaphragm on the lower end of the bone was confirmed. The clavicles were shown to have more upward than forward motion, and to move less than either sternum or ribs.

The ribs move upward more decidedly than the sternum, their rotation outwards permitting greater freedom. Their forward push is very considerable, greater than that of the sternum, often equalling or exceeding their upward rise. The upper ribs have sometimes greater forward thrust than the lower; there is no regular series in this respect in the different ribs, but, on the contrary, much evidence of the independent action of the several ribs.

The greatest amount of outward motion takes place on the rib about midway between the vertebral column and the sternum.

The measurements obtained by means of the stethometer show:—

1. That the upward dimensions of the movement are sufficiently accounted for by the upward rise of the ribs, their chord-length being taken as radius, their vertebral attachments as centres.

It may be shown also mathematically that the curving of the dorsal and lumbar regions of the spine in natural respiration would usually interfere but slightly with this "upward" reading.

2. The outward indications are also probably to be accounted for by the simple radial rise of the costal ends of the costal cartilages, the sternal articulation being taken as centre.

3. The "forward" movement is, however, much more complex. It could not arise from any simple alteration in the obliquity of the ribs:—

a. Because there is no constant relation to be discovered between the amounts of "forward" and of upward movement; and it is possible, voluntarily, at one time to produce an excess of forward, at another of upward motion.

b. Because the angles made by the ribs with the spine are not such as to permit of the amount of forward movement recorded by the stethometer. This fact is proved by measurements of these angles as found in anatomical drawings, in skeletons, and in the living subject.

In the latter the amount of forward push not accounted for by the simple rise of the rib was found to be often as great as 0·5 in. for the fifth rib, and 0·7 in. for the third rib. One example out of many of these cases is here given.

Case.—Male, *æt.* 32. Strong, healthy; 6 feet in height.

Gross diameter over 3rd rib	8½ in.
Gross diameter over 5th rib	9½ in.
Estimated chord-length of the 3rd rib 6·75 in., of 5th rib..	7·65 in.
Angle formed by the right 3rd rib during expiration.....	63°
Angle formed by the right 5th rib during expiration.....	61°
Inspiratory angle of the right 3rd rib	78°
Inspiratory angle of the right 5th rib	75°

	Forward.	Upward.
Calculated motion of the end of the 3rd rib	61	160
Calculated motion of the end of the 5th rib	53	140

Motion of the Ribs as observed with Stethometer.

A, when the whole back was supported	{ 3rd rib .. 135	160
	{ 5th rib .. 115	120
B, when the 3rd and 5th vertebræ alone	{ 3rd rib .. 135	160
were supported	{ 5th rib .. 115	120
Extent of forward push of 3rd rib not accounted for		0.74 in.
Extent of forward push of 5th rib not accounted for		0.62

4. The excess of forward motion is found not to be explained (a) by the *genou*-lever action of the costal cartilages with the ribs, (b) by the curvature of the spine, (c) by the backward thrust of the angles of the ribs.

5. It may be concluded, therefore, that the mechanical conditions of the thoracic machinery would prevent any forward motion to the extent observed, unless it was possible for the ribs themselves either to be inbent or straightened out.

6. It may be proved that the ribs are *capable* of being bent (a) by experiments upon freshly separated ribs, (b) upon dead subjects, (c) upon living subjects, (d) by pathological facts.

7. The means by which an alteration in the chord-lengths of the ribs could be brought about may be discovered in several directions, especially in the intrinsic thoracic muscles.

8. It may be shown that the hypothesis of either the straightening of the ribs in inspiration, or their previous inbending in expiration, (a) accounts for the extent of the forward indications of the stethometer, (b) their varying amount in the same individual in relation to the upward motion, (c) for the extraordinary extent of these forward indications in women and young children as compared with those of strong men and old people; (d) it explains the comparatively large extent of forward movement of the upper as compared with the lower ribs; (e) it is supported by the peculiar stethometric measurements obtained in various diseases of the lungs; (f) by the fact that the excess of forward movement is found to increase from the middle to the anterior end of the rib.

On the Respiratory Motive Powers.—By what means may the inbending or straightening of the ribs be accomplished?

That the forces concerned in respiration are sufficiently powerful is proved by several considerations, notably by Hutchinson and Haughton's calculations of muscular power.

The purely mechanical forces are probably not often brought into action. The effect of the extraordinary or external muscles of respira-

tion was not examined by means of the stethometer; but from Mr. Le Gros Clark's experiments it would appear that they have but little influence in forced inspiration.

With the intrinsic thoracic muscles the case is different. It is probable that, with the exception of the *levator costarum*, each of these muscles has some further power beyond that of simply raising or depressing the ribs.

The *triangularis sterni* is more especially a constrictor of the anterior part of the thorax.

The *intercostals* have very various actions assigned to them; but when all the evidence of Traube, Sibson, and others is considered, it seems most probable that the six upper ribs are raised by the external intercostals, and depressed by the internal muscles; but the ribs are not rigid bars, and hence these muscles have a further action upon them. From careful experiments upon a model with elastic vulcanite ribs, and elastic bands stretched between them in the direction of (a) the external intercostals, (b) the internal intercostals, and (c) with now the upper ribs fixed, now the lower, it was concluded that whilst modifications were introduced into their action by the last-named condition, yet that without doubt the tendency of the external intercostals was, 1st, to draw the ribs upwards; 2nd, to separate their anterior ends; 3rd, to straighten them. On the other hand, the action of the internal intercostals was, 1st, to draw the ribs downwards; 2nd, to bring their anterior extremities nearer together; 3rd, to bend them inwards. These results were explained by resolving the forces of these muscles in the directions (a) along the ribs, (b) at right angles to them.

The diaphragm may also be considered to have an important action in bending the lower ribs.

Evidence can be adduced showing the value of the stethometer used in these inquiries, both in physiological studies and in medical practice.

III. "On Linear Differential Equations."—No. VI.

By W. H. L. RUSSELL, F.R.S. Received July 30, 1872.

We now consider linear differential equations which are satisfied by the roots of an algebraical equation admitting of explicit solution. To determine in what cases the linear differential equation

$$P \frac{d^2 y}{dx^2} + Q \frac{d^{n-1} y}{dx^{n-1}} + \dots + R y = 0$$

is satisfied by assuming

$$y = \sqrt{X} + \sqrt{Y} + \sqrt{Z} + \dots$$

when X, Y, Z are rational functions of (x) . We shall commence by

supposing them also entire functions of (x) . At present we shall confine ourselves to the linear differential equation of the second order,

$$P \frac{d^2 y}{dx^2} + Q \frac{dy}{dx} + Ry = 0.$$

The most general form of solution which this equation will admit will be $y = \{X + \sqrt{Y}\}^n$. For if we put $y = \{X + \sqrt[3]{Y}\}^n$, we should have three particular integrals corresponding to the three cube roots of Y , n being supposed fractional.

Substituting in the proposed equation, we shall find, after making some reductions,

$$n(n-1)P\{X' + \frac{1}{2}Y^{-\frac{1}{2}}Y'\}^2 + nP\{X + Y^{\frac{1}{2}}\}\{X'' + \frac{1}{2}Y^{-\frac{1}{2}}Y'' - \frac{1}{4}Y^{-\frac{3}{2}}Y'^2\} \\ + nQ\{X + Y^{\frac{1}{2}}\}\{X' + \frac{1}{2}Y^{-\frac{1}{2}}Y'\} + R\{X + Y^{\frac{1}{2}}\}^2 = 0; \dots (1)$$

from whence we have

$$n(n-1)PYX'^2 + \frac{1}{4}n(n-1)PY'^2 + nPXYX'' + \frac{1}{2}nPY Y'' - \frac{n}{4}PY'^2 \\ + nQXYX' + \frac{1}{2}nQYY' + RYX^2 + RY^2 = 0, \dots (2)$$

$$n(n-1)PX'Y'Y + \frac{1}{2}nPXYY'' - \frac{1}{4}nPX Y'^2 + nPX''Y^2 \\ + \frac{1}{2}nQXY Y' + nQXY'^2 + 2RXY^2 = 0 \dots (3)$$

Let

$$P = \alpha + \beta x + \gamma x^2 + \delta x^3, \quad Q = \alpha' + \beta' x + \gamma' x^2, \quad R = \alpha'' + \beta'' x.$$

This will give us

$$X = p + qx, \quad Y = r + x.$$

For if we assumed either X or Y to be of higher dimensions, we should obtain, on equating the coefficients of the powers of (x) in 2 and 3 to zero, more equations than the total number of constants in the differential equation and assumed solution. The number of disposable constants is eleven. If we assumed X of two dimensions or Y of two dimensions, we should have more than eleven equations.

Now equate the coefficient of the highest power of (x) in (2) to zero, and we have

$$n^2\delta + n(\gamma' - \delta) + \beta'' = 0,$$

which determines n independently of the constants in X and Y .

Similarly, from (3),

$$n(n-1)\delta - \frac{n}{4}\delta + \frac{n\gamma'}{2} + n\gamma' + 2\beta'' = 0.$$

This of course becomes an equation of condition between the coefficients of the given differential equation, when we substitute the value of (n) we have found from the last equation.

To determine p, q, r , we proceed as follows:—In equation (3) put x successively equal to the three roots of the equation

$$\alpha + \beta x + \gamma x^2 + \delta x^3 = 0.$$

Then we have three equations, linear in $\frac{q}{p}, \frac{r}{p}, r$, from which these quan-

tities may be determined. But putting $x=0$ in equation (1), the equation becomes

$$n(n-1)aq^2r + na'rpq + \frac{na'r}{2} + a''rp^2 + \frac{n^2a}{4} - \frac{na}{2} + a''r^2 = 0.$$

Hence, substituting for $\frac{q}{p}$ and r the values we have just obtained, we have a linear equation to determine p^2 ; hence p, q, r are determined.

Now, suppose P, Q, R to be such that we may assume

$$X = p + qx + x^2, \quad Y = m + rx + sx^2;$$

then the terms of equation (2), which contain the two highest powers of x , are linear in Y , or we may divide it out in equating their coefficients to zero. Hence we may determine q at once.

Put for x successively in (2) four roots of the equation $P=0$; then we have four equations linear in p^2, p, m, r, s , whence we have a quadratic equation to determine p , and then m, r, s are known. We have beside a number of equations of condition.

Now suppose P, Q, R to be such that we may have

$$X = p + qx + x^2, \quad Y = r + sx;$$

then we shall find that the terms which contained the three highest powers of (x) are linear in Y ; consequently p, q , and therefore X , may be determined at once. X being known, we may obtain a series of equations easily by which the constants in Y may be determined*. But we may adopt a somewhat different method, which will be useful in many cases of this nature. Resuming the equation

$$(a + \beta x + \gamma x^2 + \delta x^3) \frac{d^2 y}{dx^2} + (a' + \beta' x + \gamma' x^2) \frac{dy}{dx} + (a'' + \beta'' x) y = 0,$$

equation (1) shows that $a + \beta x + \gamma x^2 + \delta x^3$ must vanish for every quantity which causes $X + \sqrt{Y}$ to vanish, and equation (3) that the same quantity must also vanish for every quantity which causes Y to vanish.

Let

$$a + \beta x + \gamma x^2 + \delta x^3 = \delta(\mu_1 + x)(\mu_2 + x)(\mu_3 + x).$$

* If

$$\begin{aligned} X &= p_0 + p_1 x + p_2 x^2 + \dots + x^\mu, \\ Y &= q_0 + q_1 x + q_2 x^2 + \dots + q_\mu x^\mu, \end{aligned}$$

the terms which contain the μ highest powers are linear in Y ; and therefore

$$p_1 p_2 \dots p_{\mu-1}$$

can be determined at once, commencing with $p_{\mu-1}$.

If

$$\begin{aligned} X &= p_0 + p_1 x + p_2 x^2 + \dots + x^\mu, \\ Y &= q_0 + q_1 x + q_2 x^2 + \dots + q_{\mu-1} x^{\mu-1}, \end{aligned}$$

the terms which contain the $(\mu+1)$ highest powers are linear in Y ; and therefore

$$p_0 p_1 \dots p_{\mu-1}$$

may be determined at once.—W. H. L. R., Nov. 21, 1872.

Then r must be one of the quantities μ_1, μ_2, μ_3 ; we will suppose μ_1 . Moreover, we must have

$$p - q\mu_2 + \sqrt{r - \mu_2} = 0, \quad p - q\mu_3 + \sqrt{r - \mu_3} = 0,$$

or

$$p - q\mu_2 + \sqrt{\mu_1 - \mu_2} = 0, \quad p - q\mu_3 + \sqrt{\mu_1 - \mu_3} = 0;$$

whence

$$p = \frac{\mu_2 \sqrt{\mu_1 - \mu_2} - \mu_3 \sqrt{\mu_1 - \mu_3}}{\mu_3 - \mu_2},$$

$$q = \frac{\sqrt{\mu_1 - \mu_2} - \sqrt{\mu_1 - \mu_3}}{\mu_3 - \mu_2}.$$

Generally, if

$$P \frac{d^n y}{dx^n} + Q \frac{d^{n-1} y}{dx^{n-1}} + \dots + R y = 0$$

be a linear equation, admitting a solution of the form

$$y = \sqrt[n]{X + \sqrt[n]{Y} + \sqrt[n]{Z} + \dots},$$

every quantity which makes $X + \sqrt[n]{Y} + \sqrt[n]{Z} + \dots$ to vanish will make P vanish; every quantity which makes $Y + \sqrt[n]{Z} + \dots$ to vanish will make P vanish; every quantity which makes $Z + \dots$ to vanish will make P vanish. This principle is of course of the greatest use in determining the form of this function.

The most general form of irrational solution for a linear differential equation of the third order will be $\{X + \sqrt{Y}\}^n$ or $\{X + \sqrt[3]{Y}\}^n$; for one of the fourth order we shall have

$$\{X + \sqrt{Y}\}^n, \{X + \sqrt[3]{Y}\}^n, \{X + \sqrt{Y} + \sqrt{Z}\}^n, \{X + \sqrt{Y} + \sqrt[3]{Z}\}^n, \\ \{X + \sqrt[4]{Y}\}^n,$$

where (n) is supposed to be fractional.

In all these cases the determination of (n) will be independent of the constants in X, Y, Z . For if we expand the proposed forms of solution in descending powers of (x) , substitute in the differential equation, and equate the coefficient of the highest power of (x) to zero, we obtain an equation to determine (n) not involving these constants.

If X, Y, Z, \dots are rational fractions, this method will not apply; but since the factors of the denominators of these fractions, not regarding their powers, must also be factors of P , we may proceed as follows:—Let $x - a = z$ be one of the factors of P , substitute $x = a + z$ in the given differential equation, and let $-m$ be the greatest negative value of μ , obtained by substituting $y = z^m + Bz^{m+1} \dots$ in the differential equation, and equating the lowest term of the result to zero. Again, let $x - b = z$, and let $-r$ be the greatest negative value of μ , obtained by substituting $x = b + z$ in like manner. Then, if we put $v = (x - a)^m (x - b)^r \dots y$, we may obtain an equation whose irrational solution will be free from negative factors.

We have seen that if $P e^{\omega}$, when substituted for y , satisfies a linear differential equation, ω is an invariant, that is, it remains the same function of x , and the constants involved in the equation whatever numerical value we are able to assign to these constants, on the supposition that P and ω are rational functions of (x) . We shall call this the "principle of index invariance," and proceed to show that it is true when ω is irrational. Let

$$(a + \beta x + \gamma x^2) \frac{d^2 y}{dx^2} + (a' + \beta' x + \gamma' x^2 + \delta' x^3) \frac{dy}{dx} + (a'' + \beta'' x + \gamma'' x^2 + \delta'' x^3) y = 0$$

be a differential equation; to find the condition that it may be satisfied by $y = P e^{\int \omega dx}$, where ω is irrational. The irrational quantity in ω must be of the form \sqrt{X} , otherwise the equation would admit of more than two particular integrals. Moreover the factors of X must be factors of $a + \beta x + \gamma x^2$. We will suppose $X = a + \beta x + \gamma x^2$. Hence we may assume

$$\omega = \mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2};$$

for if we expand ω in descending powers of (x) , and substitute the resulting value of y in the differential equation, we shall easily see that it can have no power higher than the first. μ , ν , and ρ are of course constants to be determined, and the double sign of the radical will give rise to four equations instead of two, which will imply an equation of condition.

We now substitute $y = P e^{\int \omega dx}$, where P is an algebraical function of (x) , in the linear differential equation, and thus obtain

$$\begin{aligned} (a + \beta x + \gamma x^2) \left\{ \frac{d^2 P}{dx^2} + 2(\mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2}) \frac{dP}{dx} \right. \\ \left. + (\mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2})^2 P + (\mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2})' P \right\} \\ + (a' + \beta' x + \gamma' x^2 + \delta' x^3) \left\{ \frac{dP}{dx} + (\mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2}) P \right\} \\ + (a'' + \beta'' x + \gamma'' x^2 + \delta'' x^3) P = 0. \end{aligned}$$

The coefficients of the two highest powers of (x) must be equated to zero, that is to say, the coefficients of the two highest powers of the multiplier of P must be equated to zero.

The following terms of that multiplier will be sufficient for our purpose:—

$$\begin{aligned} \delta'' x^3 + (\gamma' x^2 + \delta' x^3)(\mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2}) \\ + (\beta x + \gamma x^2)(\mu + \nu x + \rho \sqrt{a + \beta x + \gamma x^2})^2, \end{aligned}$$

or

$$\begin{aligned} \delta'' x^3 + (\gamma' x^2 + \delta' x^3)(\mu + \nu x + \rho x \sqrt{\gamma(1 + \frac{\beta}{2\gamma x} + \dots)}) + (\beta x + \gamma x^2) \\ \left\{ \mu^2 + 2\mu\nu x + \nu^2 x^2 + 2\rho(\mu + \nu x)x \sqrt{\gamma(1 + \frac{\beta}{2\gamma x} + \dots)} + \rho^2(a + \beta x + \gamma x^2) \right\}. \end{aligned}$$

IV. "On Linear Differential Equations."—No. VII.

By W. H. L. RUSSELL, F.R.S. Received October 24, 1872.

I am desirous to conclude this series of papers with some remarks on the solutions of differential equations considered as transcendents. I shall take the linear differential equation of the second order,

$$(\alpha + \beta x + \gamma x^2) \frac{d^2 y}{dx^2} + (\alpha' + \beta' x + \gamma' x^2) \frac{dy}{dx} + (\alpha'' + \beta'' x + \gamma'' x^2) y = 0,$$

which will be sufficient, as it will be seen at once that similar investigations apply generally.

Let

$$y = u_0 + u_1 x + u_2 x^2 + \dots + u_n x^n + \dots$$

We propose to investigate the general law of the convergence of this series. The general term of the series is given by the equation

$$\begin{aligned} (n+2)(n+1)\alpha u_{n+2} + (n+1)n\beta u_{n+1} + n(n-1)\gamma u_n \\ + (n+1)\alpha' u_{n+1} + n\beta' u_n + (n-1)\gamma' u_{n-1} \\ + \alpha'' u_n + \beta'' u_{n-1} + \gamma'' u_{n-2} = 0. \end{aligned}$$

As (n) increases without limit, this equation becomes

$$(n+2)(n+1)\alpha u_{n+2} + (n+1)n\beta u_{n+1} + n(n-1)\gamma u_n = 0,$$

or more simply,

$$\alpha u_{n+2} + \beta u_{n+1} + \gamma u_n = 0.$$

This may be written

$$\alpha \frac{u_{n+2}}{u_{n+1}} + \beta \frac{u_{n+1}}{u_n} + \gamma = 0.$$

Now let $\frac{u_{n+1}}{u_n}$ converge, as (n) increases without limit, to a certain quantity ρ , then $\frac{u_{n+2}}{u_{n+1}}$ will also converge toward the same quantity ρ , and ρ will be given by the equation

$$\alpha \rho^2 + \beta \rho + \gamma = 0.$$

Let ρ_1, ρ_2 be the roots of this equation, ρ_1 the greatest root; then the series $u_0 + u_1 x + u_2 x^2 + \dots$ will be convergent if $\frac{u_{n+1} x}{u_n}$ is in the limit less than unity, or if x is less than $\frac{1}{\rho_1}$. For large values of (x) we must proceed as follows:—

Let

$$y = u_0 + \frac{u_1}{x} + \frac{u_2}{x^2} + \dots + \frac{u_n}{x^n} + \dots$$

Then, substituting in the differential equation,

$$\begin{aligned} (n-2)(n-1)\alpha u_{n-2} + n(n-1)\beta u_{n-1} + n(n+1)\gamma u_n \\ - (n-1)\alpha' u_{n-1} - n\beta' u_n - (n+1)\gamma' u_{n+1} \\ + \alpha'' u_n + \beta'' u_{n+1} + \gamma'' u_{n+2} = 0. \end{aligned}$$

As (n) increases without limit, this equation may be written

$$\alpha u_{n-2} + \beta u_{n-1} + \gamma u_n = 0,$$

or

$$\alpha + \beta \cdot \frac{u_{n-1}}{u_{n-2}} + \gamma \cdot \frac{u_{n-1}}{u_{n-2}} \cdot \frac{u_n}{u_{n-1}} = 0.$$

Let $\frac{u_n}{u_{n-1}}$ converge to a certain quantity ρ , then ρ will be given by the equation

$$\alpha + \beta\rho + \gamma\rho^2 = 0.$$

Let ρ_1 be the greatest root of this equation; then the series will be convergent if $\frac{u_n}{u_{n-1}x}$ is less than unity, or if x be greater than ρ_1 .

It appears from my last paper that in certain cases a quantity of the form

$$\sqrt[n]{a+bx} + \sqrt[n]{c+ex}$$

may be regarded as a solution of a differential equation of the form here considered. Hence we are able, by means of the principles given in this paper, to represent, under these circumstances,

$$\int dx \sqrt[n]{a+bx} + \sqrt[n]{c+ex}$$

by a converging series when (x) does not exceed an ascertainable value. This method evidently admits of a great variety of applications.

November 30, 1872.

ANNIVERSARY MEETING.

Sir GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

Mr. Abel, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total receipts during the past year, including a balance of £28 2s. 2d. carried from the preceding year, amount to £4308 14s. 8d., and that the total expenditure in the same period amounts to £3860 17s. 10d., leaving a balance at the Bankers of £424 0s. 3d., and £23 16s. 7d. in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists :—

Fellows deceased since the last Anniversary.

On the Home List.

William Baird, M.D.
 Sir John Bowring, LL.D.
 Major-General Francis Rawdon
 Chesney, R.A., D.C.L.
 George E. Day, M.D.
 James Alexander Gordon, M.D.
 Augustus Bozzi Granville, M.D.
 George Robert Gray, F.L.S.
 Major Robert Wolseley Haig, R.A.
 James Heygate, M.D.
 Henry Beaumont Leeson, M.A.,
 M.D.
 William Lowther, Earl of Lonsdale,
 M.A.

The Rev. Canon Henry Moseley,
 M.A., D.C.L.
 Robert Patterson.
 Sir Thomas Phillips, Bart., M.A.
 The Venerable Archdeacon John
 Henry Pratt, M.A.
 Edward Ayshford Sanford.
 Frederic Carpenter Skey, F.R.C.S.
 Sir Andrew Smith, K.C.B., M.D.
 Colonel William Henry Sykes.
 Richard Westmacott, R.A.
 Robert Wight, M.D.
 Lieut.-General Thomas Wood.

On the Foreign List.

Charles Eugène Delaunay.

Defaulter.

Robert Gordon Latham, M.D.

Change of Name and Title.

James Orchard Halliwell	to	James Orchard Phillipps.
Sir Roundell Palmer	to	Lord Selborne.

Fellows elected since the last Anniversary.

The Right Hon. George Joachim
 Goschen.
 Prof. William Grylls Adams, M.A.
 Andrew Leith Adams, M.B.
 Frederick Le Gros Clark, F.R.C.S.
 Prof. John Cleland, M.D.
 Prof. Michael Foster, M.D.
 Prof. Wilson Fox, M.D.
 Arthur Gamgee, M.D.

Rev. Thomas Hincks, B.A.
 Prof. William Stanley Jevons, M.A.
 Prof. George Johnson, M.D.
 Prof. Thomas Rupert Jones.
 Major Thomas George Montgo-
 merie, R.E.
 Edward Latham Ormerod, M.D.
 Edward John Routh, M.A.
 William James Russell, Ph.D.

The President then addressed the Society as follows :—

GENTLEMEN,

IN commencing the Annual Address which is customary at the Society's Anniversary Meeting, I will first allude to our local position. The Society's Officers have reason to believe that, before the next Anniversary Meeting, we may be established in the new apartments now in preparation for us in this Palace. We hope to find in them rooms for our Meetings and for our Books more convenient than those which we at present occupy, and to be placed in advantageous proximity to the other Scientific Societies of the Metropolis.

On the *personnel* of the Society, the proper accounts will soon be placed before you, drawn up under the superintendence of your Secretaries. But I must allude to one loss, the accidental death of our Foreign Member, M. Delaunay. Science has, I fear, sustained a serious injury by this calamity. It is known to all who have given attention to the progress of Gravitational Astronomy, that M. Delaunay had, with incredible labour (well exhibited in several ponderous volumes), produced a work on the lunar motions which was never surpassed in order, and, I believe, never equalled in exactitude. We were looking for the completion of this undertaking, by careful investigations of the secular equations and equations of very long period on which Chronology in particular depends, and also by the formation of Lunar Tables in a convenient form. There is reason to hope that these works have been far advanced and may yet be recovered ; but I have no positive information to that effect. M. Delaunay, adopting and extending in the Paris Observatory all the labours of his distinguished predecessor, had advanced far with an admirable series of systematic records of topographical meteorology and other local measures in France.

I regret to announce that Dr. Sharpey finds it necessary, in justice not only to his own health, but also to his estimate of the powers which he is able to devote to the service of the Society, to resign the office of Secretary which he has long held with so much advantage to the Society. Every Fellow of the Society, but more especially those who have had the pleasure of serving on Council with Dr. Sharpey, will, I am sure, sympathize with me in regret that we lose his services, and still more that it is from the cause which I have mentioned. If, however, the Society should see fit to confirm the Council's recommendation of appointment of Professor Huxley in his place, I am confident that Dr. Sharpey will be one of the first to acknowledge that he transmits his office to no unworthy successor.

Among the labours distributed among Fellows of the Society of which the results do not appear in the Philosophical Transactions are the

following:—The Government-Grant Committee, the Library Committee, the Donation Committee, and the Scientific Relief Committee have all been active in their several functions. The Catalogue of Scientific Papers to the year 1863 is completed, and will long remain as a testimonial to the judgment and care of those who have superintended and completed it. So strong is the sense of the Council on its value (a value to which I can personally testify), that they have determined to carry it on to 1873.

Of the subjects which have been referred by the Government to the Royal Society, and on which the President and Council, assisted by Committees of the Society, have made Reports, the first in importance is undoubtedly the course and employment of the Expedition which is now leaving our shores, for an almost complete circumnavigation of the globe, and for a very extensive series of observations, bearing principally upon natural history, but partly also on ocean-currents, ocean-depths, and progressive geology. It is not too much to anticipate that the series of suggestions offered by the Committee which has given its attention to these subjects will long be studied by the promoters of future expeditions as guides for their own conduct.—Next is to be mentioned the consideration of an application, from the German Scientific Bodies, for the establishment of a self-registering tide-gauge on the island of Heligoland, to serve for the definition of a zero of elevation for the continent of Europe with supposed greater accuracy than any that can be obtained on the continental shores. The President and Council offer their tribute of respect to the long-sighted view, and the breadth of the considerations, which, as might be expected, have distinguished the enterprise desired by their German brethren. At the same time they have felt it their duty to indicate a point of tidal theory on which their opinions do not harmonize with those that suggested the observations in question; and they have also alluded to the very great practical difficulties in carrying out the proposed construction. In this state they have submitted the question to the British Government.—It is known that an International Commission has been sitting in Paris, for the purpose of establishing with accuracy the length of the Mètre, and for the distribution of accurate copies; and, with the concurrence of the British Government, your Foreign Secretary and the Warden of Standards have taken an active part in the deliberations; and your President would probably have joined them if his official employments had permitted it. I think it imperative on me to state that the British Government gave their assent only on the express understanding that they could take no part in the Commission if it displayed any propagandist intention. Speaking as the representative of the body who had best considered this subject, namely the Standards' Commission now dormant, I can say, as their unanimous opinion, that they deprecate the slightest interference with national usages; but they

recognize the great importance of an accurate international system which, like the Latin of the middle ages, enables men of science in all countries to speak the same language; and for this international character they think the Metrical system singularly well adapted.—In connexion with this subject I may state that the “Parliamentary Copy” of the National Standard of Length, which is intrusted to the care of the Royal Society, has been examined and found to be in excellent condition: a very small modification has been made in its supports, to meet the suggestion of a possible cause of minute injury in the contacts of the bar with its bearing-rollers.—As connected with the general science of the country, I ought not to omit to remind you that your Home Secretaries hold important posts in the Official Scientific Commission which has not yet terminated its labours.

I would next allude to the scientific subjects which within the last year have been introduced at your public meetings, and of which the greater part will be further published in your ‘Transactions’ or your ‘Proceedings.’ And as demanding my first notice in every point of view, whether as referring to the name of the writer or to the subject of which he treats, I cite the paper by Sir Edward Sabine on the measures of the Magnetic Elements in the northern regions of our globe; a worthy termination to an unequalled series of arranged results of observation, which must be adopted as the foundation for all future theoretical investigations of terrestrial magnetism. In connexion with this, I mention Captain Evans’s collection of the values of magnetic declination on numerous points of the British coasts, the magnetic survey of the eastern districts of France by Messrs. Perry and Sidgreaves, and Mr. Chambers’s determination of the magnetic elements and their changes, and of the lunar inequalities of magnetic declination, at Bombay. At the last Anniversary of the Society attention was called by my predecessor to a paper by Dr. Hornstein on the existence of a magnetical inequality whose period is sensibly the same as the length of the sun’s synodical revolution, and which therefore seems to show that different parts of the sun’s surface produce different effects on the magnet. The examinations of observations and the discussions of them within this Society have confirmed Dr. Hornstein’s observations for the year in which he observed; and they have added this remarkable result, that in each year a species of inequality appears to have been sufficiently recurrent with the sun’s revolution to show itself clearly through accidental irregularities, but that the law of the inequality varies greatly from year to year.—In Chemistry we have various communications, rather on details than on principles. Mr. Schuster’s paper on the spectrum of nitrogen points out a cause of occasional errors in spectroscopic inferences. A late communication from Sir B. C. Brodie appears to prove that ozone is an allotropic form of oxygen in which three volumes of oxygen are condensed

into two, and which may exhibit three forms of action.—In Palæontology we have Professor Owen's paper on the earlier Australian traces of that wonderful class of animals (the marsupials) which connect ancient Oxfordshire with modern Australia; Professor Williamson on the organization of certain fossil plants; and the Report by Mr. Prestwich on the exploration of Brixham Cave, referring in some measure to the habits of former man as well as former wild beasts.—In Oceanic science, the Report by Dr. Carpenter on the currents in the neighbourhood of the Straits of Gibraltar (in sequence of preceding papers) embodies important facts, and has raised some discussions. Alluding only by a word to the discoveries of deep channels in the bottom of the sea and of water of different temperatures in them, I think it right to state, as my opinion, that the flow of surface-water from the places of high temperature, and the return of deep water to the same, are certain in theory and are supported by observation.—Mr. Stone's determination of the velocity of sound is free from the effects of a long-prevailing error (the combination of two senses), and is probably one of the best yet made.—In Astronomy our communications are rich: Mr. De La Rue and his fellow labourers have continued their researches upon the influence of Planets on the Sun; Mr. Huggins has employed the telescope, wisely provided by a former Council of the Society, and the spectroscope, on the spectra of Encke's Comet and of the nebula of Orion (leading us more and more distinctly to the idea that these bodies are gaseous and in some measure self-luminous), and on that astonishing result of modern science, the measure of the approach or recess of the "fixed" stars. It is a striking thing to see the stars of a constellation so well known to every one as is "Charles's Wain" separated into two classes, of approaching and receding; and also to find that the same separation is indicated by the characters of their proper motions. An investigation of the value of the coefficient of aberration, when the star is viewed through a telescope whose tube is filled with water, may have served to remove some doubts on the optical theory of aberration, and to allay some anxieties on the reduction of astronomical observations.

I think that the opinion of the scientific world would be, that the proceedings of the Society during the past year will bear comparison with those of any other year.

Before closing my Address, I think that I may employ a few sentences in noticing some of the steps which have been made in science in the world exterior to the Society. In Geography, the most exciting of all is Mr. Stanley's discovery of Livingstone, and the intelligence of the explorations made by him and other travellers in South Africa. But they tend greatly to sharpen our curiosity on other points. Are the lakes which have been visited so many independent lakes, or are several of them portions of one great lake? Do these mighty waters reach the

sea by the Nile or by the Congo? Opinion is advancing fast in the direction that they are tributaries of the Congo; that we have, in fact, "rounded" the basin of the Nile-heads, and perhaps those of some other rivers. The observations of Dr. Schweinfurth in a more northerly district appear to support this idea. But, however strong may be the reasons for this conclusion, the world will scarcely be satisfied without local verification.—Some additions have been made to our knowledge of the coasts and seas of Novai Zemlai and other polar regions by American and other expeditions, especially that of Rosenthal; but nothing of striking character has been discovered.—The German Association for the *Europäische Gradmessung* continues its labours actively; its last Reports contain details of Geodesy from Norway to Italy, and exhibit the comparison of various standards of length.—French geodetists have proposed the extension of the great French meridional arc to Algeria, employing a triangulation with sides of about 200 miles.—In the progress of our great Indian Survey, Major Montgomerie (charged with the superintendence in Colonel Walker's absence) proposed to himself the problem of surveying "the triangular space lying between the Indus and its great Caubul tributary, which is bounded on the north by the Hindoo-Koosh and Mustagh ranges." This has been generally accomplished, and appears to have opened up the geography of an almost unknown and difficult country. It is understood that Russian surveys, though yet distant, are advancing from the opposite side.—The melancholy account of the death of Captain Basevi, while engaged in pendulum-experiments among the Himalaya Mountains, was given by my predecessor to the last Annual Meeting. An officer has been named to continue the observations, but the actual work is not yet begun.—Much attention has been called to a strange error in the measure of the elevation above the sea assigned to the centre of gravity of continents and islands. Colonel Sir Henry James has given probably the first accurate measure of surface-height (from which that of the centre of gravity may be inferred) on a portion of Scotland.—Perhaps I may consider it not wholly unconnected with this subject to state that the galvanic telegraph now extends (accidents only excepted) to the south of Australia. I have some pleasure in remarking that the gentleman who has attached the last and most difficult link to this chain was Supernumerary Computer and Junior Assistant at the Observatory of Greenwich, and was nominated by me for construction of the first telegraph at Adelaide.

The year's progress in Geology has consisted principally, I believe, in increasing the number and the accuracy of the observations of special facts, and in giving careful attention to the details of palæontology.

Magnetism continues to advance with slow but, I believe, certain steps. Twice in this year it has happened that unusual outbursts on the sun's

surface have been observed (by Father Secchi in Italy, and by Professor Young in America), and that on each occasion there has been a sudden magnetic disturbance registered on the Greenwich photographic sheets, suspiciously near in time. It may ultimately be necessary to establish the proper means for sun-observations in Australia or New Zealand, in order to ascertain whether similar agreements of time occur during the hours of European night.

Meteorology, if we could judge only from the number of observatories and the activity with which their observations are carried on, is making prodigious strides. But I doubt greatly whether science really gains any thing from millions of observations which are published with very few steps of reduction.

The principal works of the Meteorological Office, the examination of ships' logs and the deduction of inferences from them, are incessantly continued; the results are classified by degree-squares, and a specimen of a ten-degree square has been circulated for public opinion; also a detailed exhibition of the state of the North Atlantic for eleven days. Much light is thrown on the shift of the trade-wind zones. Daily weather-charts are issued, exchanged, and sold to a considerable number, and the "drum" signals are exhibited at 128 stations.

Astronomy has made great advances. Adverting, first, to the instruments.—It appears to be certain that in the Great Melbourne Telescope the principal difficulties are overcome, and the instrument is actually well and successfully employed on the objects for which it is especially intended.—The 25-inch refractor at Gateshead, the property of Mr. B. S. Newall, has been examined carefully by the most distinguished of northern astronomers, and its definition has been pronounced to be perfect.—The spectroscope has undergone various changes of form, all in the direction of increasing its prismatic separation without injuring its practical application in other respects.—Adverting, secondly, to the observations and reductions, such as preceded the introduction of the spectroscope.—The too long-delayed reduction of observations at the Cape of Good Hope has been urged vigorously by the present Astronomer, Mr. Stone; those for 1858 have lately appeared, and others will probably follow soon, the current work of the present time being still maintained. In the mean time observations have been actively made and reduced by Mr. Ellery at Melbourne. There is now good prospect of the promotion of accurate astronomy in the Southern Hemisphere.—But it is principally in the results of spectroscopic observations that the great steps have been made. Much has been done in the examination of Nebulæ, Comets, and Auroral and Zodiacal light; but the greatest share of attention has been directed to the constitution of the Sun, chiefly as revealed by the observations made in total eclipses. (It would be wrong to omit to mention that Mr.

Pogson has shown that an annular eclipse is equally available.) I cannot pretend now to describe all the characteristics of the four strata of the Sun's atmosphere, or the evidence of their containing various terrestrial components, or the proofs that they are in different states of temperature; nor can I describe the phenomena of the corona, or those in the neighbourhood of the solar maculæ. Much also has been done by spectroscopic examination of the sun without eclipse; and a spectroscopic society in Italy has employed itself principally on that object. The Lords Commissioners of the Treasury have sanctioned the establishment, at the Royal Observatory of Greenwich, of photoheliographic observations (to be made, during the Transit-of-Venus Expedition, with the Kew Instrument, by permission of your Council) and of spectroscopic observations: the former system, it may be expected, will be in regular action within a few months; the latter scarcely admits of being carried on in official routine, but will be occasionally active.—Preparations are far advanced for publishing a nearly complete report of the Eclipse of 1870; the observations of that of 1871 are not yet entirely collected.—Nearly all the Governments of the civilized world are engaged in arrangements for observation of the Transit of Venus in 1874. As regards the British preparations, the original scheme of observations was confined to eye-observation of the entry and the departure of Venus at the sun's limb, which I regard as the most accurate of all; but, considering especially the risk of weather or accident, it appeared desirable to be prepared with a method for utilizing the whole duration of Transit; and, at the instance of the Board of Visitors of the Greenwich Observatory, the Treasury have sanctioned the fitting up of a photoheliograph of the best class at each of the five British stations. We are indebted to Mr. De La Rue for the superintendence of the construction and verification of these instruments. Several foreign astronomers have turned their attention to the employment of the heliometer for the same purpose.

In Chemistry, there has been much work on details, but scarcely leading to any great principle. The theories which have mainly guided the observations appear to be those of isomerism, chemical structure, thermo-chemical research, and physiological chemistry.

The attention given to Botany has been in some measure differently apportioned to different states. In those which contain within their own limits no very great variety of terrestrial or climatic circumstances, accurate examination and classification have been studied; but in the more extended countries, and most of all in our own territory, with its enormous colonial extension, the questions of plant-distribution and general biology have been examined, and attention to the structure of fossil plants has increased. The theory of fertilization of plants by insects has gained some assent, and the necessity of observing entomology in connexion with plant-distribution is partially recognized.

In Medicine, I learn that researches on the injurious effects of abnormal temperature of the body (as in fevers), and on the way of neutralizing them, have been prosecuted with favourable results. The Sphygmograph has, I believe, been successfully employed as an instrument of diagnosis. The alteration of the form of blood-corpuscles under special circumstances appears to have attracted attention.

I may well appeal to the enumeration which I have made for proof of the assertion that science is not standing still.

For convenience, I will take the present opportunity of making a statement, of personal character, which in strictness ought to be offered at a later stage of this day's proceedings.

The Council of the Society, in their circulated house-list of Officers proposed for the Session now commencing, have done me the honour again to offer my name as President. Should this be confirmed, Gentlemen, by your votes, I will, to the best of my powers, endeavour to discharge for a period the duties of the office. But experience has convinced me that it was no unfounded fear which I expressed to the deputation of the Council who first conveyed to me the flattering invitation to the Presidency, that official and other circumstances might make it difficult for me to give proper attention to the interests of the Society. I will therefore ask you, Gentlemen, to permit me in any case to lay down my office at the next Anniversary Meeting. It may be possible, if business threatens to press me with the same severity in the autumn of 1873 as in 1872, that I may desire to be liberated from my engagement at an earlier date; this, however, I shall not contemplate so long as I can avoid doing so. Meantime I will express to the Society my feeling that the position in which they have placed me is the proudest which a man of science in this country can occupy; and that the labour which accompanies it, so far as I am able to support it, is only a just debt from me to the Society.

I proceed now to announce the award of the Medals.

The Copley Medal has been awarded to Professor Friedrich Wöhler, of Göttingen, For. Memb. R.S., for his numerous contributions to the Science of Chemistry; and more especially for his researches on the products of the decomposition of Cyanogen by Ammonia; on the Derivatives of Uric Acid; on the Benzoyl Series; on Aluminium (discovered by him), Yttrium, and Beryllium; on Boron, Silicon, and their compounds; on Titanium; and on Meteoric Stones.

PROFESSOR MILLER,

In the name of the Council, I request that you will transmit this

Medal to Professor Wöhler, and that you will assure him that his long-continued services to the science of Chemistry command our deep respect.

A Royal Medal has been awarded to Mr. Henry John Carter, F.R.S., for his researches in Palæontology and Zoology, on the Infusoria and Rhizopoda, and the root-cell of the *Chara*; but more particularly for his inquiries into the Natural History of the Spongiadæ.

PROFESSOR STOKES,

In the absence of Mr. Carter, I have much pleasure in delivering to you, for transmission to him, the Medal awarded to him by the Council of the Royal Society, in acknowledgment of his long-continued and valuable services to Physiology, and more particularly of his creating an almost new science as regards the Spongiadæ.

A Royal Medal has been awarded to Professor Thomas Anderson, M.D., for his investigations on the Organic Bases of Dippel's Animal Oil; on Codeine; on the Crystallized Constituents of Opium; on Piperin and on Papaverin; and for his researches in Physiological and Agricultural Chemistry.

PROFESSOR STOKES,

In Dr. Anderson's absence, I request you to state to him that I am happy to deliver to him, through your hands, the Medal which the Council has awarded to him, in recognition of his numerous and valuable contributions to Physiological Chemistry.

The Rumford Medal has been awarded to Anders Jonas Ångström, For. Memb. R.S., for his researches on Spectral Analysis. The limitation in time attached to the adjudication of the Rumford Medal compels the Council to connect this award with the latest of M. Ångström's published memoirs; but they are not insensible to the circumstance that this is but the termination of a long and valuable series.

M. Ångström's researches generally are described with great clearness in the late President's Address at the Anniversary Meeting of 1870. To the subjects there mentioned I have only to add a mathematical theory of the Conduction of Heat.

PROFESSOR MILLER,

Have the goodness to transmit this Medal to M. Angström, with the assurance of our regard for himself and of the great value which we place on his researches.

On the motion of General Smythe, seconded by Colonel Yorke, it was resolved,—“That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed.”

The Statutes relating to the election of the Council and Officers having been read, and Dr. Gladstone and Dr. George Harley having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year :—

President.—Sir George Biddell Airy, K.C.B., M.A., D.C.L., LL.D. (Astronomer Royal).

Treasurer.—William Spottiswoode, M.A.

Secretaries.— { Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.
 { Prof. Thomas Henry Huxley, LL.D.

Foreign Secretary.—Prof. William Hallowes Miller, M.A., LL.D.

Other Members of the Council.—George James Allman, M.D. ; Sir B. C. Brodie, Bart., M.A., D.C.L. ; George Busk, F.R.C.S. ; Prof. Robert Belamy Clifton, M.A. ; James Fergusson, D.C.L. ; Thomas Archer Hirst, Ph.D. ; J. Dalton Hooker, C.B., D.C.L., LL.D. ; Joseph Prestwich, F.G.S. ; Rear-Admiral G. H. Richards, C.B. ; Prof. H. Enfield Roscoe, B.A., Ph.D. ; Philip Lutley Sclater, M.A. ; William Sharpey, M.D., LL.D. ; Francis Sibson, M.D. ; Major-Gen. R. Strachey, B.E., C.S.I. ; Isaac Todhunter, M.A. ; Sir Charles Wheatstone, D.C.L.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Com- pounders.	£4 yearly.	Total.
November 30, 1871.	4	49	278	264	595
Since elected			+6	+10	+16
Since compounded . .			+2	—2	
Since deceased		—1	—8	—14	—23
Defaulter				—1	—1
November 30, 1872.	4	48	278	257	587

Receipts and Payments of the Royal Society from December 1, 1871, to November 30, 1872.

	£	s.	d.
Balance on hand	28	2	2
Annual Contributions, Admission Fees, and Compositions	1616	0	0
Rents	262	2	8
Dividends	1494	14	6
Ditto, Trust Funds	314	2	2
Sale of Transactions, Proceedings, &c.	542	14	2
Miscellaneous	50	19	0

	£	s.	d.
Balance due to Bankers	101	19	9
Salaries, Wages, and Pension	1060	11	4
The Scientific Catalogue	183	5	0
Spectroscopes	124	9	0
Books for the Library and Binding	269	9	5
Printing Transactions and Proceedings, Paper, Binding, Engraving, and Lithography	1626	9	1
General Expenses (as per Table subjoined)	362	16	3
Soirée Expenses	56	9	2
Donation Fund	25	0	0
Wintringham Fund	35	5	0
Copley Medal Fund	4	15	7
Davy Medal Fund	0	10	6
W. K. Parker, Bakerian Lecture	4	0	0
Dr. Stebbing, Fairchild Lecture	2	18	9
Croonian Lecture, Poor of St. James' Parish	2	19	0

75

8

10

3860

17

10

Balance at Bank

424

0

3

Balance of Catalogue Account

23

1

9

" Petty Cash Account

0

14

10

£4308

14

8

W. SPOTTISWOODE, Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
 Estate at Acton, Middlesex (34 A. 2 R. 27½ P.), £109 10s. per annum.
 Fee Farm near Lewes, Sussex, rent £19 4s. per annum.
 One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
 £14,000 Reduced 3 per Cent. Annuities.
 £29,569 15s. 7d. Consolidated Bank Annuities.
 £513 9s. 8d. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
 £590 Madras Guaranteed 5 per Cent. Railway Stock—Davy Medal Fund.
 £10,000 Italian Irrigation Bonds—The Giessot Trust.

		£ s. d.	
Investments up to July 1872, New 3 per Cent. Annuities	6328	11 2
" " Metropolitan 3½ Consols	100	0 0
Dr.		<u>£6428 11 2</u>	

	£	s.	d.
Balance	281	7	6
Dividends	191	18	8
Donation	10	0	0
	<u>£483 6 2</u>		

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1872.

	£	s.	d.
Annual Contributions	1076	0	0
Admission Fees	160	0	0
Compositions	380	0	0
Rents	282	2	8
Dividends (exclusive of Trust Funds)	1494	14	6
Sale of Transactions, Proceedings, &c.	542	14	2
Miscellaneous	50	19	0
	<u>3986 10 4</u>		
Income available for the Year ending Nov. 30, 1872		
Expenditure in the Year ending Nov. 30, 1872		
	<u>£181 1 4</u>		
Excess of Income over Expenditure in the Year ending Nov. 30, 1872		

	£	s.	d.
Balance due to Bankers	101	19	9
Salaries, Wages, and Pension	1060	11	4
Spectroscopes	124	9	0
The Scientific Catalogue	183	5	0
Books for the Library	138	10	4
Binding ditto	130	19	1
Printing Transactions, Part II. 1871 and Part I. 1872	382	0	6
Ditto Proceedings, Nos. 130-137	317	1	10
Ditto Miscellaneous	92	7	3
Paper for Transactions and Proceedings	173	3	6
Binding and Stitching ditto	84	6	3
Engraving and Lithography	577	9	9
Source Expenses	33	13	1
Fittings, Cleaning, and Repairs	45	14	1
Miscellaneous Expenses	72	9	0
Coal, Lighting, and Gas Repairs	45	1	1
Law Expenses	20	5	5
Tea Expenses	20	1	6
Fire Insurance	18	16	6
Taxes	14	9	0
Advertising	18	7	6
Surveyor's Charges	46	17	1
Postage, Parcels, and Petty Charges	25	0	0
Acton Church, Donation	2	0	0
Mablethorpe Schools, Donation	362	16	3
	<u>£3785 9 0</u>		

W. SPOTTISWOODE, Treasurer.

Account of the appropriation of the sum of £1000 annually voted by Parliament to the Royal Society (the Government Grant), to be employed in aiding the advancement of Science (continued from Vol. XX. p. 60).

1872.

1. T. R. Fraser, for an Investigation of the Antagonism between Physostigma and Atropia	£50
2. H. E. Armstrong, for Investigation of the Nitro-derivatives of Sulphuric Anhydride	50
3. Prof. W. C. Williamson, for continuation of Researches on the Organization of the Fossil Plants of the Coal-measures	20
4. Prof. Duncan, for a Research on the Minute Anatomy and Physiology of the Actinozoa	25
5. B. H. Scott, for Experiments on various forms of Anemometers	50
6. M. Hall, for Apparatus for Deep-sea Dredging	50
7. C. Greville Williams, for an Investigation of Beryls and Emeralds	100
8. J. H. Collins, for a Microscopic and Physical Examination of Cornish Rocks	25
9. G. Gore, for further Researches into the Fluorides, and for Electrodynanic, Magnetic, and Chemical Experiments	150
10. Dr. C. B. A. Wright, for continuation of Researches into the History of the Opium Alkaloids, and an Investigation of Narcine and Papaverine	50
11. R. J. Friswell, for an Investigation of the Compounds of the Platinocyanides	20
12. H. J. Carter, for aid in prosecuting Researches on the Spongiadæ, on which he has been long engaged	50
13. Treasurer R. S., on behalf of the Circumnavigation Committee to meet possible outlay for Instruments for the Circumnavigation Expedition	250
14. H. Willett, for Instruments to ascertain the Temperature of the Rocks traversed in the Exploratory Boring in the Sub-Wealden Formations near Battle	20
15. Sir William Thomson, for the reduction of Tidal Observations	100
16. J. N. Lockyer, for the payment of an Assistant in Solar Observations, and for expenses incident upon the application of photography to that object	150
	<hr/> £1160

<i>Dr.</i>	£	s.	d.			£	s.	d.	<i>Cr.</i>
To balance on hand, Nov. 30, 1871 ..	1031	8	8		By appropriations as above	1160	0	0	
To Grant from Treas- ury (1872).....	1000	0	0		Balance on hand, Nov. 30, 1872	1140	0	1	
Repayments:—									
Prof. Stokes £175	0	0							
Dr. Andrews	73	17	4						
		248	17	4					
Interest		19	14	1					
	£2300	0	1			£2300	0	1	

Account of Grant from the Donation Fund in 1872.

Prof. P. G. Tait, towards the expense of Investigations in Thermo-electricity	£25
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Presents received November 21, 1872.

Transactions.

- Berlin** :—Physikalische Gesellschaft. Die Fortschritte der Physik im Jahre 1868. Jahrgang 24. 8vo. 1872. Namen- und Sach-Register, Band I. bis XX. 8vo. 1872. The Society.
- Bologna** :—Accademia delle Scienze dell' Istituto. Memorie. Serie Terza. Tomo I., II. fasc. 1. Indici Generali dei dieci tomi della seconda serie. 4to. 1871-72. Rendiconti delle Sessioni. Anno Accademico 1871-72. 8vo. *Bologna* 1872. The Academy.
- Cherbourg** :—Société Nationale des Sciences Naturelles. Mémoires. Tome XVI. 8vo. *Paris* 1871-72. The Society.
- Devonshire Association for the Advancement of Science, Literature, and Art.** Report and Transactions. Exeter, July 1872. Vol. V. Part 1. 8vo. *Plymouth* 1872. The Association.
- Edinburgh** :—Geological Society. Transactions. Vol. II. Part 1. 8vo. *Edinburgh* 1872. The Society.
- Kiel** :—Universität. Schriften aus dem Jahre 1871. Band XVIII. 4to. *Kiel* 1872. The University.
- London** :—Institution of Civil Engineers. Minutes of Proceedings, Session 1871-72. Vol. XXXIII., XXXIV. 8vo. *London* 1872. The Institution.

Transactions (*continued*).

Linnean Society. Transactions. Vol. XXVIII. Part 2; Vol. XXIX. Part 1. 4to. 1872. Proceedings, Session 1871-72, pp. xxix-cxii. 8vo. Journal. Zoology. Vol. XI. No. 55. 8vo. 1872. Additions to the Library, 1871-72. List of Fellows, 1871. 8vo.

The Society.

Zoological Society. Transactions. Vol. VIII. Part 2. 4to. 1872. Proceedings of the Scientific Meetings for 1872. Part 1. 8vo. Revised List of the Vertebrated Animals now or lately living in the Gardens. 8vo. 1872. Catalogue of the Library. 8vo. 1872.

The Society.

Lund:—Universitet. Årsskrift, 1869, 1870. 4to. *Lund*.

The University.

Lyon:—Académie des Sciences, Belles Lettres et Arts. Mémoires. Classe des Sciences, Tome XVIII.; Classe des Lettres, Tome XIV. roy. 8vo. *Lyon* 1868-71.

The Academy.

Société Impériale d'Agriculture, Histoire Naturelle et Arts Utiles. Annales. Quatrième série. Tome I., II. 1868-69. roy. 8vo. *Lyon* 1869-70.

The Society.

Paris:—Muséum d'Histoire Naturelle. Nouvelles Archives. Tome VII., VIII. 4to. *Paris* 1870-71.

The Museum.

Penzance:—Royal Geological Society of Cornwall. The 56th, 57th, and 58th Annual Reports. 8vo. *Penzance* 1872.

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Quebec:—Literary and Historical Society. Transactions. Session of 1871-72. 8vo. *Quebec* 1872.

The Society.

St. Petersburg:—Académie Impériale des Sciences. Mémoires. Tome XVII. No. 11, 12; XVIII. No. 1-7. Bulletin. Tome XVII. No. 1-3. 4to. 1871-72.

The Academy.

Turin:—R. Accademia delle Scienze. Atti. Vol. VII. disp. 1-7. 8vo. *Torino* 1871-72.

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Upsala:—Universitet. Årsskrift, 1871. 8vo. *Upsala*.

The University.

Vienna:—K. Akademie der Wissenschaften. Denkschriften. Band XXXI. 4to. *Wien* 1872. Sitzungsberichte. Math.-nat. Classe, Erste Abth. Band LXIV. Heft 1-5. Zweite Abth. Band LXIV. Heft 3-6. Phil.-hist. Classe, Band LXVIII. Heft 2-4; Band LXIX. Heft 1-3. 8vo. 1871.

The Academy.

Observations, Reports, &c.

Cape of Good Hope. Royal Observatory. Results of Astronomical Observations made in the years 1857, 1858, reduced by E. J. Stone, F.R.S. 8vo. *Cape Town* 1872. E. J. Stone, F.R.S.

Observations, Reports, &c. (*continued*).

London :—Army Medical Department. Report for the year 1870.
Vol. XII. 8vo. *London* 1872. The Department.

British Museum. Catalogue of the Specimens of Hemiptera Heteroptera in the Collection of the British Museum, by F. Walker.
Part 5. 8vo. 1872. Appendix to the Catalogue of Shield Repetiles: Part 1, by J. E. Gray. 4to. 1872. The Trustees.

Incorporated Law Society. Catalogue of the Mendham Collection of Books and Pamphlets. 8vo. *London* 1871. The Society.

Meteorological Office. Daily Weather Reports. Jan.—June 1872. fol. Quarterly Weather Report. Part 1. Jan.—March 1871. 4to. 1872. Discussion of the Meteorology of the part of the Atlantic lying north of 30° N. for the eleven days ending 8th of February, 1870. Charts and Diagrams. 4to. 1872. January Chart of Meteorological Data for No. 3 Square. Remarks to accompany Monthly Charts. 4to. 1872. Report of the Meteorological Committee of the Royal Society for the year ending 31st December, 1871. 8vo. 1872. The Office.

Royal College of Surgeons. Calendar, July 11, 1872. 8vo. *London*.
Annual Report of the Conservator to the Museum Committee. 8vo. 1872. The College.

University College. Calendar, Session 1872–73. 8vo. *London* 1872. The College.

Veterinary Department. Report for the year 1871. fol. *London* 1872. The Department.

Melbourne :—Observatory. Eighth Report of the Board of Visitors. fol. 1872. The Board of Visitors.

Oxford :—Radcliffe Observatory. Results of Astronomical and Meteorological Observations made in the year 1869 under the superintendence of the Rev. R. Main. Vol. XXIX. roy. 8vo. *Oxford* 1872. The Radcliffe Trustees.

Southampton :—Ordnance Survey Office. Ordnance Survey of the Peninsula of Sinai made by Captains C. W. Wilson and H. S. Palmer under the direction of Sir Henry James. Part 1, Account of the Survey; Part 2, Maps, Plans, and Sections; Part 3, Photographic Views. In 5 vols. fol. *Southampton* 1869.

The First Commissioner of H.M. Works, &c

Anderson (J.) The Strength of Materials and Structures. 12mo. *London* 1872. The Author.

Attfield (J.) Chemistry: General, Medical, and Pharmaceutical; including the Chemistry of the British Pharmacopœia. Fourth Edition. 8vo. *London* 1872. The Author.

- Baily (W. H.) Figures of Characteristic British Fossils, with Descriptive Remarks. Part 1-3. 8vo. *London* 1867-71. The Author.
- Darwin (C.), F.R.S. The Expression of the Emotions in Man and Animals. 12mo. *London* 1872. The Author.
- Fayrer (J.) The Thanatophidia of India, being a Description of the Venomous Snakes of the Indian Peninsula. fol. *London* 1872. The Author.
- Monro (Alex.), F.R.S. The Structure and Physiology of Fishes explained and compared with those of Man and other Animals. fol. *Edinburgh* 1785. Dr. W. T. Iliff.
- Ormerod (E. L.), F.R.S. Clinical Observations on the Pathology and Treatment of Continued Fever. 8vo. *London* 1848. British Social Wasps: an Introduction to their Anatomy and Physiology, Architecture, and General Natural History. 12mo. *London* 1868. The Author.
- Owen (Prof. R.), F.R.S. On the Carpal Copulatory Spines (or supposed Horn) of the Iguanodon. 4to. *London* 1872. The Author.
- Pole (Wm.), F.R.S. Iron as a Material of Construction. 12mo. *London* 1872. The Author.
- Ramsay (A. C.), F.R.S. The Physical Geology and Geography of Great Britain. Third Edition. 8vo. *London* 1872. The Author.
- Müller (F. Max) Rig-Veda-Sanhita, the Sacred Hymns of the Brahmans, together with the Commentary of Sayanacharya. Vol. V. 4to. *London* 1872. The Secretary of State for India.

Engraved Portrait of Rear-Admiral Sir James Clark Ross, D.C.L., F.R.S., from the original picture by Stephen Pearce, in the Royal Hospital, Greenwich. Admiral Ommanney, F.R.S.

*Report of the Kew Committee for the Fifteen Months
ending October 31, 1872.*

THE Kew Observatory was handed over to the President and Council of the Royal Society by the General Committee of the British Association at the Edinburgh Meeting in 1871. Since that time the operations of the establishment have been conducted under the directions of the Committee nominated by the President and Council of the Royal Society.

The most important modifications which have been made in the staff of the Observatory have been:—first, the appointment of Mr. Samuel Jeffery as Superintendent, in the place of Dr. Balfour Stewart, F.R.S., who had resigned his office previous to the severance of the connexion between the Observatory and the British Association; and, secondly, the resignation of Mr. Robert Beckley, who had been for nearly eighteen years Mechanician, such an official being no longer required. His connexion with the Observatory is not, however, entirely at an end, for he has been appointed Consulting Mechanician with a retaining fee of Ten Pounds per annum.

Mr. Robert H. Scott, F.R.S., has consented, at the request of the Committee, to act as their Honorary Secretary.

Magnetic Work.—The series of automatic records of the several Magnetographs, viz. Declinometer, Horizontal Force, and Vertical Force instruments, have been continued, and the independent absolute determinations have been, as usual, made monthly. This latter duty has been, as heretofore, performed by Mr. G. M. Whipple, B.Sc., first Assistant, who also takes charge of the General Magnetic Work, in which he has the assistance of Mr. Cullum. The salaries of these two gentlemen, whose time is chiefly devoted to magnetic work, amounted during the period under consideration to £308 2s. 6d., leaving a balance of about £300 out of the sum of £600 received from the Royal Society to meet the general expenses (£2084 12s. 9d.) of the Observatory. £767 18s. 5d. of this amount has been defrayed by the Meteorological Office and other sources, such as the £300 received for the payment of arrears from the

British Association (the greater part of which has been already expended), fees for verification of instruments, and payments for new instruments for foreign observatories, leaving a balance in hand of £85 5s. 6d. on the 31st of October.

The state of the Magnetic Reductions, as exhibited in the Report of the British Association for 1871, remains unaltered, with the exception of the Tabulations of Declination, which have been extended to the end of the year 1871. The discussion of the results has been undertaken by the Chairman of the Committee, Sir E. Sabine, who has made arrangements for the constant attendance at Kew since November 1871 of two sergeants of the Royal Artillery who were formerly located in his office at Woolwich. Accommodation has been provided for them at Kew by partitioning off a portion of the Transit Room.

Meteorological Work.—The several self-recording instruments, registering respectively the Pressure, Temperature, Vapour-tension, Rainfall, and Wind, have been maintained in constant action under the superintendence of Mr. T. W. Baker, second Assistant, aided by Mr. Foster; and the daily standard eye observations for control of the photographic records have been made regularly.

The instrumental traces with hourly tabulated values are sent monthly to the Meteorological Office as in former years. The Barograms and Thermograms are printed off in duplicate, and one copy is preserved at Kew. As regards the Anemograms and Rain-records, the copy has been obtained by the method of tracing.

In addition to the regular work of Kew as a Magnetical and Meteorological Observatory, the duty of examining and checking the work of all the seven Self-recording Observatories in connexion with the Meteorological Office has been carried on in accordance with the method described in the Report of the British Association for 1869. This portion of the work has been performed by Messrs. Page and Rigby.

The only change as regards the Meteorological Instruments has been the repair of the shafts which connected the Anemograph on the Dome with the registering apparatus. About the middle of June it was discovered that the old tubular shafts were split, and they were therefore replaced by new ones.

Photoheliograph.—This instrument was regularly worked, as in former years, up to the end of February 1872, at which epoch the period expired which was originally fixed by Mr. De La Rue for the continuance of the observations at the expense of the Royal Society Government-Grant Fund. The observations were afterwards carried on up to the end of March, with the object of fully including ten years. The measurements and reductions of the sun-pictures have been continued at the expense of Mr. De La Rue, and will be completed during the ensuing year. A scale of equal parts, 15 feet in length, has, with the sanction of Her Majesty's Office of Works, been erected temporarily on the Pagoda at

the Royal Gardens, Kew, likewise at the expense of Mr. De La Rue*. This is being photographed by the Kew Photoheliograph for the purpose of determining the optical distortion (if any) of that instrument, in order to furnish the final corrections to the reductions of the sun-pictures made with it.

A Heliograph for Pulkowa has been placed in the Dome, and photographs have been taken of the scale in order to determine its optical character. This instrument has since been removed; but the stages and preparations for future instruments remain *in situ*, and will be used for testing the other instruments to be employed for the observations of the transit of Venus.

Since March, when the sun-photograms were discontinued, eye observations of the sun, after the method of Hofrath Schwabe, have been made with a telescope of $2\frac{3}{4}$ inches aperture (lent by Sir E. Sabine) by Mr. Foster, in order that the observations for connecting sun-spots with magnetic phenomena might not drop through until photographic records are taken up on a permanent footing.

Electrometer.—This instrument, designed by Sir W. Thomson, F.R.S., was provided by the Meteorological Committee in 1869, but has never been systematically in operation. It was dismantled early in the year, in order to be removed to another position. Previous to its re-erection some modifications are required; and accordingly, on a recent visit to London of the maker, Mr. White, of Glasgow, that gentleman came down to Kew (September 28) and removed a considerable portion of the apparatus in order to effect the necessary alterations at home.

Verifications.—This department of the Observatory has been in full activity during the year. The following Magnetic Instruments have been verified to obtain new constants:—

A Unifilar for the North-American Boundary Commission.

A Unifilar, a Dip-circle, and Fox's Instrument, for H.M.S. 'Challenger,' the first two having been formerly used on board H.M.S. 'Nassau.'

* Disbursements by Mr. Warren De La Rue from February to October 31, 1872:—

	£	s.	d.	£	s.	d.
Paid to Mr. Loewy for measuring and reducing the position of spots to the end of October	75	0	0			
„ Mr. Whipple for area measurements.....	20	0	0			
				95	0	0
„ Mr. Munro for Scale	30	7	6			
„ Mr. Jeffery for erection of the Scale	20	0	0			
				50	7	6
„ Mr. Whipple for Chemicals	10	0	0			
„ „ Photographing Scale	5	0	0			
„ Mudd and Son for experiments with Dry Plates	2	3	6			
„ Mr. T. Coleman for Photographic Printing.....	0	15	6			
				17	19	0
				£163	6	6

In addition a considerable number of Instruments have been ordered on commission for several foreign and other observatories, and transmitted to their respective destinations after verification at Kew. The list is as follows :—

A complete set of Magnetographs, on the pattern of the Kew instruments, for Prof. C. Jelinek, Director of the Central-Anstalt für Meteorologie und Erdmagnetismus, Hohe Warte, Vienna.

A Unifilar for Prof. Iwan Smirnow, Kasan.

A Unifilar for Prof. Balfour Stewart, F.R.S., Owens College, Manchester.

A Dip-circle for the Survey Office, Lisbon.

A Dip-circle for Prof. Karlinski, Observatory, Cracow.

A Dip-circle for Prof. Balfour Stewart, F.R.S., Owens College, Manchester.

2 Dip-needles for Prof. Jelinek, Vienna, together with alterations to 4 others connected with a Repsold's Dip-circle.

The Meteorological Instruments which have been verified have been as follows :—

Barometers, Standards	34
„ Marine and Station	73
	<hr/>
	107
 Aneroids	 17
Thermometers, Ordinary Meteorological ..	1219
„ Boiling-point Standards ..	31
„ Mountain	16
„ Clinical	1395
	<hr/>
	2661

In addition, 12 Standard Thermometers have been calibrated and divided at Kew, and 12 Maximum and 12 Minimum Thermometers have been verified, the latter being tested down to the freezing-point of mercury. These instruments have been ordered by Mr. G. T. Kingston for the meteorological organization of the Dominion of Canada.

Hydrometers

12

The following miscellaneous instruments have also been verified :—

Theodolite

1

Sextants

3

Cathetometer Scale

1

Rain-gauge

1, with 2 graduated glasses.

Evaporating Dish

1, with 1 graduated glass.

Robinson's Dial Anemometers .

4

An Anemometer, of a similar construction to the instruments belonging

to the Meteorological Committee, but modified by Mr. Beckley, on the suggestion of Mr. De La Rue, so as to express the velocity by hourly traces, the pencil returning at the expiration of each hour to the zero-line, and also to admit of a range to the extent of 100 miles in the hour, has been tested and found to work satisfactorily.

Various other Anemometers of different constructions have been under experiment; but from the limitation of the space forming the Observatory garden, by trees, &c., it was not practicable to test thoroughly their respective merits.

Waxed paper for photographic purposes has been supplied to the India Office ($2\frac{1}{2}$ reams), the Meteorological Office (5 reams), and the Lisbon Observatory ($\frac{1}{2}$ ream).

Instruction in the use of Magnetical or Meteorological Instruments has been given to the following gentlemen:—

Prof. H. F. Blanford, Meteorological Reporter to the Government of Bengal, in Meteorological work.

The Right Rev. the Bishop of Rupert's Land, in Meteorological work.

Charles Carpmæl, Esq., B.A., Assistant to Mr. Kingston, Magnetic Observatory, Toronto, Canada, in both Magnetical and Meteorological work.

Three Officers of the North-American Boundary Commission, in Magnetical work.

Three Officers of H.M.S. 'Challenger,' in Magnetical work.

Instruments and Apparatus.—The several pieces of Apparatus &c. enumerated in the Inventory (Appendix II. of the Report of the British Association, 1871) have been handed over to the Kew Committee, and are in their charge.

In the month of July instructions were received from the Foreign Office to deliver to Capt. Anderson, R.E., an officer attached to the North-American Boundary Commission, the Magnetic Instruments used by the previous Commission, which had been deposited at Kew by the late Major R. W. Haig, B.A. Accordingly the following instruments were given up to Capt. Anderson:—A portable Unifilar, a Dip-circle, and an Azimuth Compass.

The last-named instrument, although deposited by Major Haig, was not mentioned in the letter from the Foreign Office, and it was therefore issued to Capt. Anderson on loan.

The Dip-circle mentioned in the last Kew Report of the British Association has been lent to His Excellency Major-General Lefroy, Governor of Bermuda.

A portable Dip-instrument, which had been lent to the Astronomer Royal for a brief period, has been returned by him.

Buildings.—Some slight alterations have been made in the main buildings in order to afford additional accommodation.

A small closet in the Testing-room has been converted into a Lavatory.

An office has been provided for the two Sergeants, B.A., who at Kew, as already reported, by the subdivision of the old Transit about midway with a panel partition. The expense of this has been defrayed by Sir E. Sabine.

The roofs of the two outer Magnetic houses have been re-covered felt, as it was found that they could not be kept perfectly water-tight by a coating of tar.

Early in the year, on the application of the Committee, H.M. of Woods and Forests sent workmen to clean and paint the interior of the Observatory, as far as the ground-floor. This work has been finished and the Office has undertaken to complete the basement story in the course of the next year.

Library.—The Books enumerated in Appendix III. to the Report of the Kew Committee of the British Association for 1871 have been deposited at Kew.

A most valuable donation was received in July from the Committee of the Athenæum Club, consisting of 77 volumes of books, chiefly relating to the Observations.

From Sir E. Sabine a number of volumes of the 4th Memoirs of the Meteorological Results from the Colonial Observatories have been received; and also a very considerable amount of MS. documents, including, among others, the original observations from which the results above mentioned have been derived.

A large number of valuable books have been deposited at Kew by Sir E. Sabine, who has intimated his intention to present them ultimately to the Observatory as a foundation of a Library.

Staff.—The Staff employed at Kew is as follows:—Mr. Jeffery, *Superintendent*; G. M. Whipple, B.Sc., *First Assistant*; Baker, *Second Assistant*; F. J. Page, A. J. Rigby, J. E. J. Foster, F. Figg.

The last-named gentleman was substituted in May for T. H. who had resigned in April.

Visitors.—The Observatory has been honoured during the year by the presence of several scientific men of eminence; amongst these are mentioned:—

Professor E. Alluard, Clermont, Puy de Dôme, Director of the Meteorological Observatory on the Puy de Dôme.

Professor Guthrie, F.R.S., with twenty-five Teachers (Science Department).

M. W. de Fonvielle, on behalf of the Minister of Public Instruction, France, to inquire into the subject of Lightning-conductors.

M. G. Lemoine, Secretary of the Société Météorologique de France.

M. Otto von Struve, Director of the Imperial Observatory at Pulkowa, Russia, with reference to the testing of the Pulkowa heliograph.

The Committee append to the Report a statement of the total income and expenditure of the Observatory up to October 31, 1872.

Report of the Kew Committee.

		125	6	0	Furniture and Fittings		96	2	9
Vernation Fees		4	4	0	Printing and Stationery		32	19	5
Fees for Instruction of Observers		42	7	2	Postages		9	10	4
Sale of Waxed Paper		1	0	0	Messenger and Housekeeper		62	0	0
Standard Thermometer		7	10	0	Night Observer		3	13	6
Services of Assistant		209	0	7	Portage and Contingencies		38	0	2
Purchase of Instruments for Correspondents		3	11	0	Instruments on Commission account		146	3	5
Sale of old material		20	13	1	Postages and Portrages, Meteorological Committee account		220	0	8
Mr. De La Rue for Sun-work		3	2	6	Preparation of Waxed Paper		13	10	2
Meteorological Office for Anemograph papers		23	15	7	Verification Department expenses		31	9	6
					Apparatus and Material		0	13	8
					Sun-work expenses		45	0	9
					Anemograph papers, Meteorological Office		22	19	1
					Dr. B. Stewart, balance as per printed statement		40	16	3
					" " of Sundry Accounts		134	19	4
					" " less Cash at Observatory		175	15	7
					Cash in hand		17	0	5
					London and Westminster Bank		50	5	9
							34	19	9
							85	5	6
							£2084	12	9
							£2084	12	9

Examined, compared with the vouchers, and found correct. (Signed) W. J. SMYTHE, Major-General, Auditor.
November 11, 1872.

ASSETS.

	£	s.	d.
By Balance as above	85	5	6
Due for Verification Fees, Meteorological	77	0	10
" " Magnetical	42	0	0
By Instruction Fees	119	0	10
Standard Thermometers sold	10	0	0
Waxed Paper sold	8	0	0
Meteorological Committee, one month's allowance	54	3	4
" " balance of Postage account	0	15	11
Sundry sums due on Commission account	54	19	3
	10	19	10
	£298	0	11

LIABILITIES.

	£	s.	d.
To Gas and Fuel	20	11	3
House expenses	3	1	11
Stationery and Printing	17	13	6
Chemicals	3	18	0
Balance	252	16	3

December 5, 1872.

Rear-Admiral G. H. RICHARDS, C.B., Vice-President, in
the Chair.

It was announced from the Chair that the President had appointed as
Vice-Presidents :—

The Treasurer.
Mr. Busk.
Dr. Hirst.
Admiral Richards.
Dr. Sibson.

The following communications were read :—

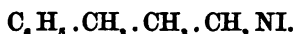
- I. "Synthesis of Aromatic Monamines by Intramolecular Atomic Interchange." By A. W. HOFMANN, M.D., LL.D., F.R.S.
Received August 22, 1872.

In a paper submitted to the German Chemical Society about a year ago, we proved (Dr. Martius and myself*) that the action of methylic alcohol on aniline chlorhydrate at a high temperature and under pressure, far from yielding exclusively methyl- and dimethylaniline, as had been formerly believed, is capable of causing methylation of the phenyl group, and thus producing quite a series of higher homologues of dimethylaniline.

If we endeavour to gain an insight into the mechanism of this reaction, we are led to assume that in the first instance the chlorhydric acid of the aniline salt gives rise to the formation of methylic chloride, which in its turn induces substitution, first in the ammonia fragment, and ultimately in the phenyl group itself. If, on the other hand, we remember that a tertiary monamine, such as must be formed by the final methylation of the ammonia fragment in aniline, when submitted to the action of an alcohol chloride, is invariably converted into an ammonium compound, it must appear rather strange that, in the process above alluded to, only tertiary, and never any quartary bases are observed.

Under these circumstances the idea very naturally suggested itself of submitting the behaviour of quartary compounds at a high temperature under pressure to an experimental investigation.

The simplest compound that could be selected for such an inquiry appeared to be trimethylphenylammonium iodide.



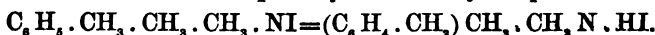
Reserving for a future communication the experimental details of this

* Hofmann and Martius, *Berichte*, 1871, p. 742.

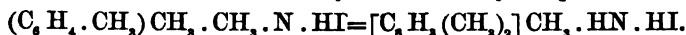
inquiry, I will limit myself for the present to a brief statement of the principal results obtained.

Leaving secondary reactions out of consideration, the transformation of the trimethylated phenylammonium iodide is represented by the following equations:—

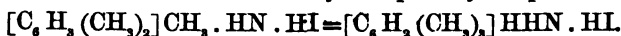
Transformation of quaternary into tertiary compound.



Transformation of tertiary into secondary compound.



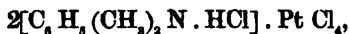
Transformation of secondary into primary compound.



Accordingly trimethylated phenylammonium iodide, when submitted to the action of heat, is transformed, in the first place, into iodhydrate of dimethylated methylophenylamine or dimethyltoluidine; this, in a second phase of the reaction, becomes iodhydrate of monomethylated dimethylophenylamine or methyl xyloidine, which in its turn is ultimately converted into iodhydrate of trimethylophenylamine, i. e. of cumidine. The essential character of the reaction is thus seen to be an intramolecular change in the position of the methyl groups. According to the duration of the process, there are incorporated in the benzol nucleus, first the methyl group of the alcohol iodide, and then successively the two methylic groups which are stationed in the ammonia fragment. The action of heat on the quaternary ammonium compound thus places at our disposal a simple means of rising from the benzol series itself to the toluol, xylol, and cumol series, or, generally (for the reaction may probably be utilized in many other cases), of passing from a less carbonated to a more carbonated series of compounds.

In carrying out the researches, the general results of which are sketched in the preceding paragraphs, rather considerable quantities of trimethylated phenylammonium iodide were consumed. This I obtained partly by methylating pure aniline with methyl iodide, partly by starting from commercial dimethylaniline, which was most liberally supplied to me by my friends Drs. Martius and Mendelssohn Bartholdy, having been specially purified for this purpose by Mr. G. Krell, by fractional distillation in the laboratory of the factory. This purified material was found to boil between 192° and 200°; and only few rectifications were necessary in order to obtain from it dimethylaniline in a state of perfect purity, identical in every respect with the base prepared by submitting trimethylated phenylammonium hydrate to distillation. Pure dimethylaniline is a liquid of 0.9553 volume weight, solidifying to a crystalline mass at + 0° 5, and boiling

at 192°. The boiling-point was repeatedly determined, since it had been erroneously stated by M. Lauth* to be 202°. The nature of the compound was ascertained by the analysis of the beautiful platinum salt



crystallizing in well-formed tables of considerable solubility.

In the early experiments trimethylated phenylammonium iodide was employed in the pure state, such as is obtained by crystallization; subsequently, however, it was found to be quite sufficient if 1 mol. of dimethylaniline was mixed with 1 mol. of methyl iodide, and the compound thus produced at once submitted to the action of heat.

The quartary iodide may be exposed to a temperature of 200° for a considerable time without undergoing any alteration; but when heated for a day to from 220° to 230°, the salt is changed, the whole crystalline compound being transformed into an amber-yellow viscid mass, exhibiting no longer a trace of crystalline structure. If the temperature be then raised to the melting-point of lead (335°), a further change is manifested by the amorphous resinous substance having solidified again to a hard mass of large radiated, generally rather coloured crystals. On opening the digestion-tubes, appreciable quantities of unflammable gas are evolved.

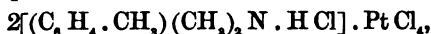
The products formed at moderate and extreme temperatures essentially differ from one another. This is seen at once when the iodhydrates produced in both cases are decomposed by alkali, and the bases thus liberated are submitted to distillation in a current of steam. The volatility of these bases shows the absence of quartary compounds; but whilst the monamines formed at moderate temperatures unite with acids to extremely soluble salts, which are scarcely to be crystallized, those which are produced at high temperatures are found to solidify to rather difficultly soluble, readily crystallizable salts with acids. The former bases exhibit the characters of *tertiary* and *secondary*, the latter ones those of *primary* monamines. Under these circumstances, it appeared desirable separately to examine the products formed in different conditions of temperature.

Examination of the Monamines formed at moderately High Temperatures.

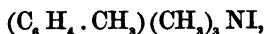
On submitting the iodhydrates formed at 220°–230° to distillation with alkali, a basic oil is obtained which, when rectified after drying over hydrate of potassium, boils between 200° and 280°. By repeated distillation the boiling-point is considerably lowered, small quantities of substances boiling beyond the range of the thermometer being separated. Finally, by far the greater portion of the bases is found to pass between 186° and 220°. This liquid consists of two varieties of dimethyltolui-

* Lauth, Bull. Soc. Chim. [2] vol. vii. p. 448.

dine, of methylxylidine, and small quantities of dimethylxylidine. Of the two dimethyltoluidines, the one has the vol. w. 0.9324, and boils constantly at 186°; the other has the vol. w. 0.9368, and boils at 205°, i. e. 19° higher than the former one. The nature of these two bases was fixed by the analysis of their platinum salts,



and also of the quartary iodide,



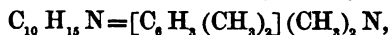
into which they were converted by the action of methyl iodide, and the platinum salts corresponding to these iodides,



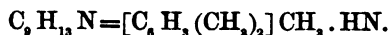
The two dimethylated toluidines here described obviously correspond to two of the three modifications of toluidine, and very probably to the two liquid modifications. Dimethyltoluidine, obtained by converting solid toluidine into the trimethylated toluylammonium iodide, and then submitting the corresponding hydrate to distillation, has a vol. w. 0.988, and boils at 210°. The substance thus obtained, the composition of which was likewise established by analysis of the platinum salt, essentially differs from the isomeric base boiling at 186°; it is less easily distinguished from the base boiling at 205°, with which more particularly it agrees in odour; in fact these two compounds exhibit only a slight difference of 5° in their boiling-points. Still I believe them to be isomeric, not identical.

It deserves to be noticed that while the boiling-point of solid toluidine (202°), by the introduction of two methyl groups, is raised by 8°, the boiling temperature of one of the liquid modifications (198°) is lowered by not less than 12°. Phenomena of this kind have been observed repeatedly in the course of this inquiry.

It has been already mentioned that, in addition to the two dimethylated toluidines, the product of the action of heat on trimethylated phenylammonium iodide contains methylxylidine. I have not been able to isolate this compound; but it was not difficult to prove its presence by the action of methyl iodide on the mixed bases. The two dimethylated toluidines are thus converted into quartary iodides; but, together with these compounds, there is formed a tertiary iodhydrate, the base of which is readily separated by distillation of the product with an alkali. The base thus liberated has the vol. w. 0.9293, and boils at 196°. Analysis of the platinum salt proved it to be dimethylated xylidine,



which previous to methylation must have obviously existed in the form of monomethylated xylidine,



The presence of methylxylidine being only indirectly proved by analysis

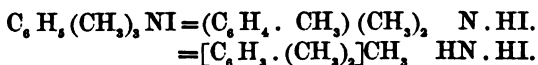
of the dimethylated base, it appeared desirable to establish the nature of the latter by additional experiments. For this purpose the tertiary monamine was converted, by means of methyl iodide, into the quaternary compound, the characters of which could not be mistaken, its composition being moreover established by analysis of the beautiful platinum salt,



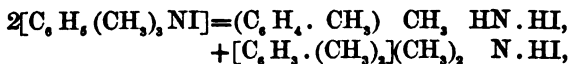
In performing these experiments I was astonished to observe how difficultly dimethylxylylidine combined with methyl iodide. Digestion at 100° produced no effect, and only by heating the mixture for many hours to a temperature of 150° combination took place, but even then only to a very small extent.

It was this indifference of dimethylxylylidine towards methyl iodide which enabled me to discover that small quantities of this compound are always formed, together with the monomethylated xylylidine, when trimethylated phenylammonium iodide is submitted to the action of heat. On treating the liquid, chiefly consisting of the two dimethylated toluidines and of monomethylated xylylidine, with methyl iodide, these bases, as I have pointed out, are converted into iodine compounds; the small quantity of dimethylxylylidine which as such exists in the liquid remains behind with the excess of methylic iodide, from which it may easily be separated by means of hydrochloric acid.

The formation of dimethylated toluidines and of monomethylated xylylidine requires no special explanation; it is due to intramolecular atomic interchange.



For the generation of dimethylxylylidine it is necessary to supply a methyl group from without. I have, however, already pointed out that, along with the principal transformation, several secondary reactions are taking place; those I hope to examine more minutely by-and-by. Dimethylxylylidine, which occurs in comparatively small quantity, obviously belongs to such a secondary change. The complementary product is probably monomethyltoluidine,



which I have not, however, as yet been able to trace.

Whilst engaged with these experiments, I have, for the sake of comparison, converted a specimen of xylylidine obtained from aniline-oil of high boiling-point into dimethylxylylidine. The xylylidine employed had a constant boiling-point at 216° . The tertiary base procured from it was observed to boil at 203° , i. e. 7° higher than the compound derived from trimethylated phenylammonium iodide; from this last derivative it differed, moreover, by combining much more readily with methyl iodide. The

quartary compound thus formed often remains liquid for days, and then suddenly solidifies into a beautiful mass of crystals.

Examination of the Monamines formed at High Temperatures.

It has been already stated that the bases into which trimethylated phenylammonium iodide is converted at very high temperatures (melting-point of lead) unmistakably exhibit the character of primary monamines. The only primary base which can arise from trimethylated phenylammonium iodide by intramolecular atomic interchange is a trimethylphenylated monamine, *i. e.* a cumidine.

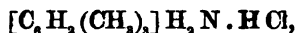


This, I may at once observe, is indeed the principal product of the reaction. It cannot, however, be wondered at that, under the influence of such extreme temperatures, many collateral changes must take place. The presence of by-products is at once perceived when the crystalline contents of the digestion-tube are submitted to distillation in a current of steam. Together with the vapour of water, a colourless oil is volatilized, consisting of hydrocarbons partly solid, partly liquid, the examination of which will form the subject of a future communication. Addition of an alkali to the liquid in the retort liberates considerable quantities of monamines, which, when dried over sodium hydrate, are observed to boil between 225° and 260°. By repeated distillation this range of boiling is still considerably expanded; at the same time, by far the largest portion of the liquid is found to pass between 217° and 230°. The primary nature not only of this main fraction, but also of the bases having both a lower and higher boiling-point, is at once manifested by the crystallizing power and insolubility of the salts which they produce. At whatever stage of the distillation a drop of the liquid passing be mixed with dilute hydrochloric or nitric acid, invariably splendid needles of chlorhydrates or nitrates are formed, the solutions of which, even when considerably diluted, solidify with platinum perchloride to double salts generally well crystallized. Another experiment rapidly indicating the primary character of these monamines may here be mentioned. On adding benzoyl chloride to the several basic fractions, much heat is evolved; and after cooling crystalline masses are produced, which are separated by water into soluble chlorhydrates and insoluble benzoyl compounds which may be crystallized from alcohol. None of the many secondary and tertiary monamines which have passed through my hands in the course of this inquiry exhibit this deportment; and accordingly benzoyl chloride may be recommended as a valuable reagent, readily applicable for primary bases. The method of recognizing primary monamines which I pointed out some time ago*, and which consists in converting them, by means of alcoholic

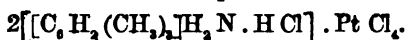
* Hofmann, D. chem. Berichte, 1870, p. 767.

potash and chloroform, into the powerfully smelling isonitriles, may also with advantage be resorted to.

The liquid boiling between 217° and 230° was separated by distillation into four fractions, each of which was then converted into a magnificently crystalline chlorhydrate. These several salts, after recrystallization, were all found to contain



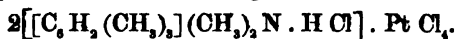
and to yield platinum salt of the composition



I was thus led to believe that the fraction boiling between 217° and 230° consisted of several isomeric cumidines; but on separating the bases from the several chlorhydrates, it was found that they all exhibited very nearly the same boiling-point. The liquid thus obtained boiled between 225° and 227° , and had the vol. w. 0.9633; it did not solidify when exposed to a temperature of -10° . I am therefore inclined to assume that only one cumidine is formed by the action of heat on trimethylated phenylammonium iodide, and that the irregularities in the boiling-point of the original fraction must be due to the presence of small quantities of impurities.

It deserves to be noticed that cumidine obtained from aniline, when heated with corrosive sublimate, yields no trace of red colouring-matter, whilst a splendid crimson is at once produced if a mixture of this base with pure aniline be treated. I reserve for a future communication the study of the colouring-matter thus obtained.

Taking into consideration the general observations recorded in the preceding paragraphs, the compound here designated as cumidine was naturally assumed to be a primary monamine. Little doubt as this conception appeared to present, it had nevertheless to be proved by experiment; for this purpose the base was submitted to methylation. Cumidine is readily acted upon by methyl iodide at the common temperature. Since it was only necessary to establish the degree of substitution, the first product of methylation was at once submitted to a second treatment; this second methylation likewise commenced at the common temperature, but had to be finished in the water-bath. The dimethylated base thus obtained had the vol. w. 0.9076: it boiled between 213° and 214° ; hence, in this case also, the insertion of two methyl groups had lowered the boiling-point. Dimethylcumidine may be cooled to -10° without solidifying: like all tertiary monamines, it forms very soluble salts, but gives a very beautiful platinum salt, containing



Remarkably enough, dimethylated cumidine exhibits the same reluctance to form a quaternary compound with methyl iodide that has already been pointed out as a peculiarity of the tertiary xylidine. But whilst in the case of dimethylxylidine, though difficultly and sparingly, combination after

all took place; all attempts with dimethylated cumidine have hitherto failed. The base was heated with methylic iodide for days in the water-bath, and ultimately even to 150° without any result. This inability of forming quartary compounds must, in one way or another, depend upon the arrangement of the material within the molecule. At all events, it deserves to be noticed that there are dimethylated xylidines and cumidines which readily combine with methyl iodide. The dimethylated bases existing in the less volatile fractions of commercial dimethylaniline all form quartary compounds without difficulty, and must therefore correspond to xylidines and cumidines which differ from those derived from trimethylated phenylammonium iodide.

In what relation stands the cumidine above described to the cumidines already known? Of the several purely methylic cumidines which are possible, two only are somewhat accurately known; these are the two bases which are derived, the one from so-called pseudocumol (obtained by treating xylitic bromide and methylic iodide with sodium), the other from mesitylol. The former cumidine is a solid fusing at 62° , and need not therefore be further considered here. Most probably the cumidine above described will prove identical with the primary monamine corresponding to mesitylol. Unfortunately mesitylamine has been hitherto so little studied, that even its boiling-point is not known. I hope next winter to examine more minutely this group of compounds.

In conclusion, I have great pleasure in expressing my best thanks to Mr. E. Mylius, Assistant in the Berlin Laboratory, for the zeal and care with which he has furthered the progress of these researches.

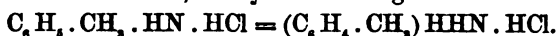
II. "Transformation of Aniline into Toluidine." By A. W. HOFMANN, M.D., Ph.D., F.R.S. Received August 22, 1872.

Whilst engaged with the experiments described in the previous paper, I have made some observations which I may be allowed briefly to mention to the Society, since the coming autumnal vacation will suspend work for a time.

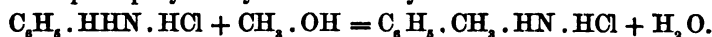
In reviewing the experience collected as to the action of high temperatures upon trimethylated phenylammonium iodide, the question naturally suggested itself whether the transposition of methyl groups, such as it occurs in the quartary compound, would take place also on heating tertiary or secondary ammonium salts. Experiment has answered this question in the affirmative.

Generally speaking the phenomena are exactly what, from the results of former observations, might have been expected. It is my intention to study next winter more soundly the reactions here sketched out; for the present I may be permitted just to allude to the happy solution of a problem with which during the last few years I had repeatedly, but always unsuccessfully, been engaged.

Salts of methylaniline (experiments were made with the chlorhydrate and iodhydrate) may be heated to 220° or 230° for hours without undergoing any alteration; but if the temperature be raised to the melting-point of lead (335°), the methyl group in the ammonia fragment passes over to the benzol nucleus, methylaniline being converted into toluidine.

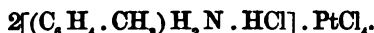


It is not necessary for this purpose to prepare methylaniline in the pure state. If a mixture of 1 molecule of pure aniline chlorhydrate and 1 molecule of methylic alcohol be heated for some hours to 230°–250°, a yellow transparent resinous mass of the consistency of honey is formed, which is principally methylaniline chlorhydrate.



After a day's exposure to a temperature of 335° the contents of the digestion-tubes are perfectly altered. In place of a transparent viscous resin, they now contain a solid beautifully crystalline mass; the secondary salt has been converted into a primary one. The crystalline mass, unless the temperature has been too high, dissolves in water almost without a residue; on addition of alkali to this solution an oily base is separated, which rises as a brownish layer to the surface of the liquid. If this oily layer be distilled in a current of steam, a colourless liquid passes, which, partly in the cooler, partly in the receiver, solidifies to a white crystalline mass of toluidine. There are but few secondary products formed in this reaction.

Toluidine thus obtained, when recrystallized from water, has the melting-point 45°. Though scarcely necessary, it was identified by a platinum determination, which gave the value corresponding to the formula



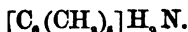
It deserves to be noticed that methylaniline iodhydrate, when heated like the chlorhydrate, yielded no solid, but liquid toluidine. I must leave it undecided which of the two liquid toluidines was formed in this case.

The observations rapidly sketched in this note invite to new experiments in a variety of directions. Is it possible to obtain by similar processes homologues of the monamines of other classes, perhaps even of some of the bases contained in the organism of plants and animals? Already I have commenced the study of naphthylamine, the behaviour of which will be particularly interesting in more than one respect. According to some observations published upon this subject, the preparation of methylated naphthylamines would appear to present some difficulty; but by working at moderate temperatures, mono- and dimethylated naphthylamine, as well as the quartary ammonium base, are readily procured. The salts of these compounds are remarkable for their power of crystallization; I have not yet found time to examine their behaviour under the influence of heat.

In conclusion I may observe also that the secondary products of the action of heat upon trimethylated phenylammonium iodide appear to be of interest. A small fraction of the bases, generated at 335°, boils at a very high temperature; when allowed to stand for some time this fraction deposits splendid crystals of a monamine which, according to the analysis of the well-crystallized chlorhydrate and platinum salt, contains



and the constitution of which, after a more minute examination, will probably prove to be expressed by the formula



Finally, there is found among these secondary products a beautifully crystallized hydrocarbon, melting at 136°, and boiling between 230° and 240°. Some combustions of this body, which require confirmation, lead to the simple formula



which determination of the gas volume weight will probably raise to



May this hydrocarbon actually be looked upon as sexmethylated benzol? If so, its oxidation promises results which will deserve minute investigation.

III. "Colouring-matters derived from Aromatic Azodiamines."

By A. W. HOFMANN, Ph.D., F.R.S., and A. GEYGER, Ph.D.

Received July 10, 1872.

II. *Safranine*.

Whilst we were engaged with the study of the blue colouring-matter produced by the action of aromatic monamines on azodiphenyldiamine our attention became directed to a beautiful red tar-pigment, which has been known for some time by the commercial name of *Safranine*, being extensively used as a substitute for safflower in dyeing silk and cotton. *Safranine* has not as yet been minutely examined; but, as far as can be judged from the scanty information we possess regarding its production, it is scarcely doubtful that this important dye must be looked upon as being the derivative of an azodiamine. The analysis of *safranine* thus promised to throw considerable light upon the nature of the compounds under examination.

The starting-point of our studies was the dye such as it occurs in commerce. The greater portion of it was supplied to us by Messrs. Tillmanns, of Crefeld. Another specimen was received from Dr. J. Wolf, and for a third one we are indebted to our friend M. Charles Girard, of Paris. Both the first-mentioned specimens were assigned to us as

produced on the large scale; the last was prepared by M. Girard himself.

Safranine occurs in commerce either as a solid body or *en pâte*. In the solid state it forms a yellowish-red powder, in which, together with considerable quantities of chalk and common salt, the chlorhydrate of a tinctorial base may be recognized. The pure dye is easily separated from crude safranine. It is only necessary to exhaust the commercial product with boiling water; on cooling, the filtrate deposits a slightly crystalline substance which, after several recrystallizations from boiling water, leaves no residue on ignition. During these operations, however, the salt undergoes perceptible alteration; with every recrystallization it becomes more soluble and less crystalline. These alterations depend upon the separation of chlorhydric acid from the salt. In fact the percentage of chlorine is found to diminish in the product of successive crystallizations; thus the product of the third contained 8.48 per cent., that of the fourth crystallization only 7.46 per cent. Addition of chlorhydric acid to the mother-liquors at once reproduces a crystalline precipitate. This instability of the chlorhydrate, and in fact, as may even now be stated, of the salts of safranine in general, has very considerably impeded the study of this body, and often materially affected the accuracy of the analytical results. In order to obtain the normal salt, the boiling liquid during the last crystallization had always to be acidified with chlorhydric acid.

Safranine chlorhydrate separates in fine reddish crystals from the acid solution on cooling; an appreciable quantity of the compound, however, remains in solution. The salt dissolves both in water and alcohol, more readily when hot than when cold; it is insoluble in ether, as also in concentrated saline solutions. The alcoholic solution, like the aqueous, is of a deep reddish-yellow colour; it exhibits a peculiar fluorescence, which in a measure recalls that of Magdala-red. Ether precipitates the salt from the alcoholic solution. Numerous analyses which we have made of this salt lead to the following two formulæ:—

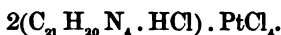


the theoretical values of which agree almost equally well with the averages of the experimental numbers. We were doubtful at first to which of these two formulæ preference should be given. Indeed the assumption of twenty atoms of carbon in the molecule of safranine had its seductions; however, the values of the second formula agree perhaps somewhat better with the results of analysis. Indeed we found invariably somewhat less nitrogen than the quantity required by the former formula, whilst it is well known that the volumetric method invariably gives an excess; so that we should have adopted the second formula, even if later experiments on the preparation of safranine had not altogether excluded the former one.

Platinum salt of safranine.—The numbers obtained in the analysis of

the chlorhydrate are confirmed by the investigation of the platinum salt. This compound is obtained by precipitating a solution of the chlorhydrate by excess of platinum perchloride; dilute hydrochloric acid must be used to wash this precipitate, as pure water appears to decompose it. The platinum salt forms a yellowish-red crystalline powder, nearly insoluble in water, alcohol, and ether.

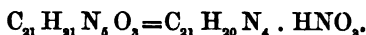
If the salt suspended in boiling water be treated with sulphhydric acid, sulphide of platinum is slowly formed, whilst unaltered chlorhydrate of safranine remains in solution. The analysis of the salt, dried at 100°, gave numbers corresponding to the formula



Free base.—In order to prepare some other salts of safranine it was necessary to obtain the free base. Remembering, when first we commenced the study of this new compound, the properties of the tinctorial ammonias in general, we endeavoured to precipitate the base from the chlorhydrate by alkalis. Ammonia, however, is without any action whatever on salts of safranine; and only in the most concentrated solutions does soda produce a precipitate which immediately redissolves on addition of water. This precipitate is obviously nothing but chlorhydrate rendered insoluble by the sodium chloride formed, or by the concentrated soda solution. Free safranine being soluble in water, there remained no other process for liberating the base than treatment of the chlorhydrate with silver oxide. On evaporating the deep yellowish-red coloured liquid obtained in this way, a deposit of red-brown crystals is formed on cooling, which while moist scarcely differ from those of the chlorhydrate, but when dried at 100° assume a greenish metallic lustre. Free safranine easily dissolves in water and alcohol, but is insoluble in ether. Addition of chlorhydric acid to the aqueous solution at once reproduces the crystalline chlorhydrate. We have not been fortunate enough to obtain free safranine in a state of perfect purity; the solution invariably retains a small quantity of silver chloride, which separates with the crystallizing product; it may be recognized by burning the base, when a small incombustible residue is left. If the chlorhydrate be reproduced from the free base, so much silver chloride is mixed with the salt, that the percentage of chlorine, found on analysis, is appreciably raised. An estimation of chlorine in the chlorhydrate thus prepared gave 10·8 per cent. instead of 9·74. Free safranine, however, may be used without difficulty in the preparation of other salts, the nitrate for instance.

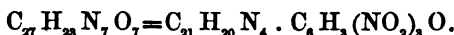
Safranine nitrate is easily obtained when an excess of dilute nitric acid is added to a hot aqueous solution of the free base. On cooling the salt crystallizes in red-brown needles, which are very insoluble in cold, but readily dissolve in boiling water. They are considerably more soluble in alcohol than in water. The nitrate is decidedly less soluble than the chlorhydrate; a solution from which the latter salt has been thrown down by chlorhydric acid still gives a precipitate with nitric acid.

The analysis of the nitrate, dried at 100°, leads to the formula



Safranine picrate.—We have finally investigated the picrate. It is obtained by simply adding an aqueous solution of picric acid to the mother-liquors of the chlorhydrate or nitrate, and washing the precipitate thus formed with water. The picrate forms red-brown needles, which are insoluble in water, alcohol, and ether.

Analysis gave results corresponding to those obtained in the investigation of the chlorhydrate, the platinum salt, and the nitrate, and leading to the formula



We have no further analytical data to offer at present. A brief notice of several other salts we have prepared may, however, still be appended.

Safranine bromhydrate is deposited on addition of bromhydric acid to a solution of the base as a crystalline powder, consisting of microscopic needles, which are so insoluble in water that the liquid from which they have been precipitated appears almost colourless. The salt is soluble in boiling water, from which it crystallizes on cooling. It is worth mentioning that, on adding bromine water to a solution of the chlorhydrate, a red crystalline precipitate is formed, which is difficultly soluble in cold water, but may recrystallize from boiling water. In this way needle-shaped crystals are obtained which, when pure, possess a green metallic lustre. They have not yet been analyzed.

The *iodhydrate*, as regards preparation and properties, is perfectly similar to the bromhydrate.

The *sulphate of safranine* is a rather soluble salt; only in the most concentrated aqueous solution of the base is a precipitate produced on addition of dilute sulphuric acid; the salt dissolves on heating the liquid, and is again deposited on cooling in the form of fine needles.

The *oxalate* behaves very similarly, but is a little less soluble than the sulphate; with acetic acid the base gives no precipitate; by spontaneous evaporation the acetate separates in rather indistinct crystals.

All the salts of safranine exhibit a very characteristic reaction; on adding chlorhydric or sulphuric acid to the solution of the salt, the red-brown colour of the liquid changes to a beautiful violet, which, on addition of more acid, becomes a deep blue, then a dark green, and finally a bright green; if the acid liquid be slowly diluted with water, these changes of colour may be observed in inverse order.

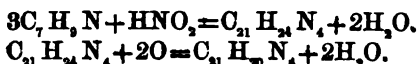
It need scarcely be mentioned that in the course of this investigation our attention has been repeatedly directed towards the preparation of safranine; it may, however, at once be stated that our experiments on this subject have yielded but scanty results. As yet but little is known regarding the preparation of safranine on a large scale. According to a

brief notice published by Mené*, safranine is obtained by treating aniline successively with nitrous and arsenic acids; this process very closely agrees with the method by which M. Girard, as he informs us, has prepared the specimen transmitted to us. We have, moreover, to thank M. Girard for pointing out that aniline of a very high boiling-point is especially adapted to the preparation of safranine.

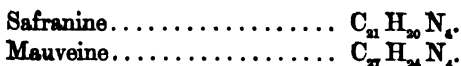
After some practice we have been able, by working according to these indications, to obtain safranine with all the properties of the commercial product. The yield, however, was always very small, an exceedingly large quantity of but little attractive by-products being invariably formed. Somewhat more satisfactory results were obtained when chromic instead of arsenic acid was used as oxidizing agent. Our experiments, though unsuccessful as regards the discovery of a good method for preparing safranine, nevertheless appear to supply some welcome indications as to the real source of the colouring-matter. Neither from pure aniline nor from solid toluidine have we been able to obtain safranine by the above-mentioned process; nor did a mixture of pure aniline and solid toluidine yield a better result. We have, however, invariably obtained safranine when liquid toluidine, boiling at 198° , was used for the experiment. This colouring-matter thus appears to become a pure derivative of toluidine, and the formula



to which analysis had led, is corroborated, as far, at all events, as the number of carbon-atoms in the molecule of safranine is concerned, in a most satisfactory manner by the formation of this body. In this formation, as in that of rosaniline and of the allied tinctorial ammonias, 3 mols. of monamine associate with a complex molecule, 3 atoms of hydrogen in which are displaced by one of nitrogen, 4 atoms of hydrogen being, moreover, removed by oxidation.



A glance at the safranine formula with its four nitrogen-atoms recalls the composition which Mr. Perkin ascribes to mauveine.



The idea suggests itself that mauveine might be phenyl safranine,

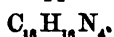


In fact safranine salts yield a violet colouring-matter when boiled for some time with aniline; again, safranine and mauveine exhibit nearly the same changes of colour under the influence of concentrated acids.

* Mené, from the *Revue Hebd. Chim. Scient. Indust.* Feb. 29, 1872, in *Chemical News*, vol. xxv. p. 215.

Further, from a short notice published some years ago by Mr. Perkin*, safranin appears to be a by-product in the preparation of mauveine.

Relations such as suggested by the above formulæ must, however, be received with great caution. The violet colouring-matter derived from safranin by phenylation has not as yet been further characterized. No great stress can be laid on the similar colour-reactions with acids exhibited by both bases, since the methylated rosanilines, on treatment with acids, also become first blue and then green. Moreover it is doubtful whether the base obtained by Mr. Perkin as a secondary product in the manufacture of mauveine really is the same safranin which we have investigated, inasmuch as some preliminary analyses have led him to a very different composition, which appears to be expressed by the formula



Finally, the composition of mauveine itself cannot be looked upon as established above all question; at least Mr. Perkin† seems still to hesitate between the formula



which appears to result from more recent experiments, and the formula

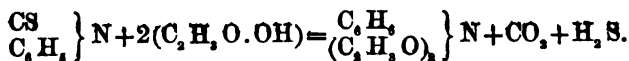


previously announced by him.

The relations here pointed out are, however, well worthy of a thorough experimental investigation. As we intend to continue this inquiry, an opportunity may present itself for their further elucidation.

IV. "New Method for producing Amides and Nitriles." By E. A. LETTS, Berlin University Laboratory. Communicated by A. W. HOFMANN, F.R.S. Received August 15, 1872.

Some time since Professor Hofmann‡ showed that phenyl mustard-oil, when acted on by acetic acid under pressure, is converted into phenyl-diacetamide, carbonic anhydride and sulphuretted hydrogen being separated.



Bearing this reaction in mind, the question arose as to how the metallic sulphocyanates would behave under similar circumstances; at Professor Hofmann's suggestion I have submitted this question to an experimental investigation.

Action of Acetic Acid on Potassium Sulphocyanate.

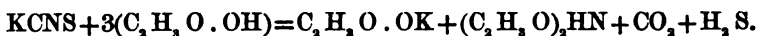
Supposing the potassium salt of sulphocyanic acid to undergo a change

* Perkin, Proc. R. Inst. of Great Britain, vol. v. p. 572.

† Perkin, *loc. cit.*

‡ Hofmann, Berichte d. Deutsch. Chem. Gesell. 1870.

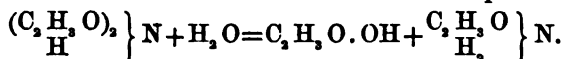
analogous to that observed with the phenyl mustard-oil, it was to be expected that 1 molecule of this body would react with 3 molecules of acetic acid to produce 1 molecule of potassium acetate and 1 molecule of diacetamide, carbonic anhydride and sulphuretted hydrogen being evolved.



The reaction, however, takes a different course.

In my first experiments the acetic acid was allowed to react on the sulphocyanate under pressure; but it soon became evident that this was unnecessary, simple digestion of the two bodies in a flask provided with an upright condenser being amply sufficient.

The powdered salt dissolves readily in the boiling acid, and an immediate and copious disengagement of gas ensues, in which carbonic anhydride and sulphuretted hydrogen may be readily recognized. Considerable time, however, elapses before the sulphocyanate is completely decomposed, some three or four days being required for a mixture of 100 grms. of potassium sulphocyanate and 180 grms. of acetic acid. At the end of this time the products of the reaction were submitted to distillation and commenced boiling at 170° – 180° ; the thermometer, however, rose rapidly to 216° ; and between this temperature and 220° the distillate solidified in the receiver to a radiating crystalline mass, which analysis showed to be pure *acetamide**. Above 220° nothing further distilled, the residue in the retort consisting wholly of potassium acetate. In what manner had acetamide been formed, instead of diacetamide expected in the reaction? It appeared not unlikely that the acetic acid employed contained some water, and that the diacetamide produced in the first instance was thus converted into the monocompound—



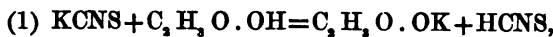
To remove any water which might possibly have been present, the acetic acid was treated with phosphoric anhydride, and the experiment with the sulphocyanate repeated; but even now acetamide was exclusively obtained.

On submitting, however, the gases evolved during the reaction to a closer examination, the formation of acetamide became at once intelligible. It was found that a large proportion of these consisted of carbonic oxysulphide (COS). To prove the presence of this compound, it was only necessary to pass the evolved gases through a bottle containing a slightly acid solution of lead, by which the sulphuretted hydrogen was retained: thus purified, they produced no further precipitate when passed through a second bottle containing the same solution; but precipitation at once took place if this were rendered alkaline by soda or ammonia.

* The liquid products passing over before 216° yield considerable quantities of the amide on fractionation; the same remark applies to the other experiments with the fatty acids to be presently described.

This is the characteristic behaviour of carbonic oxysulphide, which was further identified by its odour, great density, and inflammability.

The principal reaction that takes place when acetic acid is treated with potassium sulphocyanate accordingly is as follows:—



The sulphuretted hydrogen and carbonic anhydride, produced simultaneously with the carbonic oxysulphide, are the complements of a second reaction, the principal product of which I have no doubt is *acetonitrile*:

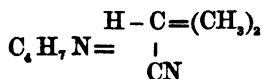


I must remark, however, that I have not actually proved the formation of this body by experiment. My investigations in the acetic series were completed before this phase of the reaction was thoroughly understood; and thus probably, owing to its low boiling-point (77°), the acetonitrile had been carried off with the stream of disengaged gases, and had escaped detection. I have not repeated the experiment, because in other series it has been easy to demonstrate the formation of the nitrile.

Action of Isobutyric Acid on Potassium Sulphocyanate.

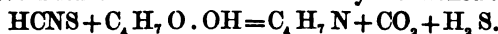
If potassium sulphocyanate be heated with isobutyric acid (which may now be readily obtained in a state of purity by oxidation of isobutylic alcohol separated from fusel-oil), the salt melts under the acid to an oily layer, from the surface of which bubbles of gas are plentifully disengaged, consisting, as in the preceding case, of carbonic oxysulphide, carbonic anhydride, and sulphuretted hydrogen. In consequence of the higher boiling-point of isobutyric acid (154°), the reaction proceeds more rapidly than with acetic acid. If the mixture be submitted to distillation when all disengagement of gas has ceased, it begins boiling a few degrees above 100° , the thermometer rapidly rising to 216° ; during this time an aromatic liquid passes over, possessing the odour of butyric acid.

Between 216° and 220° the thermometer remains tolerably stationary, the distillate solidifying in the receiver to a white crystalline mass. On fractionating the liquid portion passing over before 200° , no product can be obtained showing a constant boiling-point; but on treating it with a solution of caustic soda, an oily aromatic liquid of characteristic odour floats on the surface, which, separated by a tap-funnel and dried over calcium chloride, boils constantly between 107° and 108° . Its composition and reactions characterize this substance as isobutyronitrile:



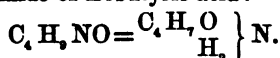
		Theory.	Experiment.
C ₄	48	68·56
H ₇	7	10·14
N	14	20·30
		<hr/>	<hr/>
		69	100·00

Boiled for some time with an alkali, this nitrile is converted into isobutyric acid and ammonia. The complementary products attending the production of this body are carbonic anhydride and sulphuretted hydrogen, the copious evolution of which has been already mentioned :



Isobutyronitrile has been prepared by Morkownikoff*. He obtained it by treating isopropyl iodide with cyanide of potassium ; but probably not in a state of purity, as he gives 80° as its boiling-point, whereas 107–108° (the number obtained by myself) approaches more closely that observed for the normal butyronitrile (114°).

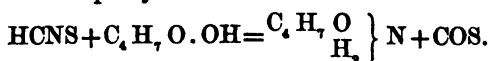
The crystalline substance before described as passing over between 216° and 220° is the amide of isobutyric acid :



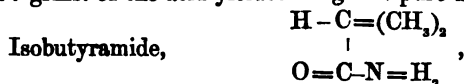
The analysis gave :—

		Theory.	Experiment.
C ₄	48	55·17
H ₇	9	10·35
N	14	16·09
O	16	18·39
		<hr/>	<hr/>
		87	100·00

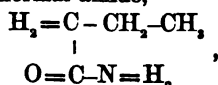
Isobutyramide forms a white crystalline mass of pleasant aromatic odour ; it fuses between 100° and 102°, and when heated somewhat above this temperature, but far short of its boiling-point, sublimes in beautiful iridescent laminae. It boils between 216° and 220°, and distils without the slightest decomposition. It dissolves readily in water and alcohol, and slightly in ether. Isobutyramide is the principal product of the action of isobutyric acid on potassium sulphocyanate :



200 grms. of the acid yielded 60 grms. pure amide.



is distinguished from the normal amide,

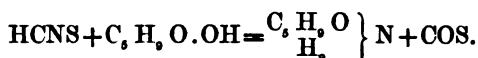
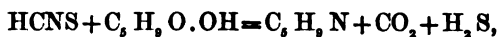


* Morkownikoff, Jahresb. xviii. 318.

by its lower melting-point. The isocompound fuses between 100° and 102° , whereas the normal butyramide melts at 115° . The boiling-point is much the same for both*.

Action of Valeric Acid on Potassium Sulphocyanate.

In this instance, too, the reaction proceeds according to the two equations—

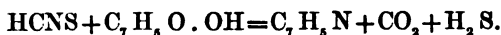


The separation and purification of the valeronitrile and valeramide are similar in all respects to those employed for the corresponding compounds in the butyric series. Valeronitrile, already investigated by other chemists, boils between 125° and 128° . By treatment with fuming nitric acid, it is converted into a crystalline substance (perhaps $\text{C}_5\text{H}_9(\text{NO}_2)\text{N}$), which I have not as yet been able to examine further.

Valeramide closely resembles isobutyramide. It is a white crystalline body of pleasant aromatic odour, recalling that of valerian-root: it is soluble in water, alcohol, and ether; in the last-mentioned liquid much more so than isobutyramide. From hot water it crystallizes in large right-angled but very thin plates. Valeramide fuses between 125° and 128° , and sublimes, in the same manner as isobutyramide, below its boiling-point, which lies between 230° and 232° . It distils without decomposition.

Action of Benzoic Acid on Potassium Sulphocyanate.

The aromatic acids of the $\text{C}_n\text{H}_{(2n-8)}\text{O}_2$ series act in an analogous manner on potassium sulphocyanate, with the difference, however, that here nearly exclusively the nitrile is formed. The production of amide is so insignificant as to be scarcely recognizable. The reaction with *benzoic acid* takes place with particular facility, according to the equation



2 mols. benzoic acid and 1 mol. potassium sulphocyanate (both in a perfectly dry condition) are placed in a retort, to the mouth of which a long wide tube is attached to serve as condenser. The apparatus thus arranged is placed vertically, and heated either in a paraffin-bath or over the naked flame. Both bodies melt, forming two layers, of which benzoic acid is the lower one. At 190° the reaction commences, carbonic anhydride and sulphuretted hydrogen being evolved. At a higher temperature the mixture enters into ebullition, and in about half an hour is converted into a solid mass. The retort is now reversed, and the contents

* It is remarkable that acetamide, butyramide, and isobutyramide all have nearly the same boiling-point, namely 216° – 220° .

strongly heated; they melt, boil, and yield a semisolid distillate. The distillation is carried on as far as possible without charring the residue, which consists of potassium benzoate (from which half the benzoic acid employed may be recovered). By addition of ammonia to the semisolid distillate any benzoic acid that has passed over is retained, whilst the nitrile distils with the water, from which it is afterwards separated, dried, and redistilled. A roughly carried out experiment yielded 50 per cent. of the theoretical quantity of nitrile in a perfectly pure condition; the loss owing to secondary reactions is amply compensated by the ease and rapidity of the operation.

Action of Cuminic Acid on Potassium Sulphocyanate.

An experiment with cuminic acid yielded very satisfactory results: cumonitrile was obtained in about the same proportions as the benzonitrile. The temperature at which the reaction commenced was here about 210° ; the nitrile was purified as in the preceding case.

Finally, an experiment was made with cinnamic acid; but although sulphuretted hydrogen was evolved, and the reaction appeared to proceed exactly as in the foregoing cases, no cinnamitrile was obtained in the liquid distillate. The cinnamic acid seemed to be decomposed into carbonic anhydride and cinnamol before being acted upon by the sulphocyanic acid.

The ease with which nitriles and amides are obtained in this manner, both in the aromatic and fatty series, induces me to hope that the new method may be of use in many cases, and perhaps, by its application to other series, give rise to bodies hitherto uninvestigated.

The tediousness of preparing the acid chloride and subsequent treatment with ammonia (for the amide), or of the amide with phosphoric anhydride (for the nitrile), in the ordinary method for producing these nitrogen compounds, is here replaced by simple digestion of the acid with sulphocyanate of potassium, bodies generally readily procurable.

- V. "Investigation of the Attraction of a Galvanic Coil on a small Magnetic Mass." By JAMES STUART, M.A., Fellow of Trinity College, Cambridge. Communicated by the President. Received July 26, 1872.

From investigations given by Ampère, we can deduce an expression for the potential U at an external point Q of a closed circular galvanic current carried by a wire of indefinitely small section. Let a be the radius of the circle; let the distance of Q from C , the centre of the circle, be r ; and let the line CQ make an angle θ with the normal to the plane of the circle. Then it can be shown that when r is less than a ,

$$U = 2\pi k \left\{ -1 + \frac{r}{a} P_1 - \frac{1}{2} \frac{r^2}{a^2} P_2 + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{r^3}{a^3} P_3 - \dots \right\};$$

and when r is greater than a ,

$$U = 2\pi k \left\{ -\frac{1}{2} \frac{a^2}{r^2} P_1 + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{a^4}{r^4} P_2 - \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6} \cdot \frac{a^6}{r^6} P_3 + \dots \right\},$$

where k depends only on the intensity of the current, and where P_1, P_2, P_3 are defined by the equation

$$\frac{1}{\sqrt{1-2x \cos \theta + x^2}} = 1 + P_1 x + P_2 x^2 + P_3 x^3 + \dots$$

If, therefore, X represents the resolved part perpendicular to the plane of the circle and towards it of the force exerted by the current on a unit of magnetism placed at Q , and if Y represent the resolved part of that force parallel to the plane of the circle and directed from its centre outwards, then

$$X = \frac{dU}{r \cdot d\theta} \sin \theta - \frac{dU}{dr} \cos \theta,$$

$$Y = \frac{dU}{r \cdot d\theta} \cos \theta + \frac{dU}{dr} \sin \theta.$$

To calculate these quantities, we know that

$$P_1 = \cos \theta,$$

$$P_2 = \frac{5}{2} \left(\cos^3 \theta - \frac{3}{5} \cos \theta \right),$$

$$P_3 = \frac{63}{8} \left(\cos^5 \theta - \frac{10}{9} \cos^3 \theta + \frac{15}{63} \cos \theta \right).$$

We shall only consider the case of those points for which r is greater than a . Substituting these values in the expression which in such instances holds for U , we have

$$U = 2\pi k \left\{ -\frac{1}{2} \cdot \frac{a^2}{r^2} \cos \theta + \frac{15}{16} \cdot \frac{a^4}{r^4} \left(\cos^3 \theta - \frac{3}{5} \cos \theta \right) - \frac{315}{128} \cdot \frac{a^6}{r^6} \left(\cos^5 \theta - \frac{10}{9} \cos^3 \theta + \frac{15}{63} \cos \theta \right) + \dots \right\}.$$

From which, after some reduction, we obtain

$$\begin{aligned} \frac{X}{2\pi k} = & -\frac{1}{2} (-1 + 3 \cos^2 \theta) \cdot \frac{a^2}{r^2} + \frac{1}{16} (9 - 90 \cos^2 \theta + 105 \cos^4 \theta) \frac{a^4}{r^4} \\ & - \frac{1}{128} (-75 + 1575 \cos^2 \theta - 4725 \cos^4 \theta + 3465 \cos^6 \theta) \frac{a^6}{r^6} \\ & + \dots \dots \dots (1) \end{aligned}$$

$$\frac{Y}{2\pi k} = \sin \theta \cdot \left\{ + \frac{3}{2} \cos \theta \cdot \frac{a^2}{r^3} - \frac{1}{16} (-27 \cos \theta + 105 \cos^3 \theta) \frac{a^4}{r^5} \right. \\ \left. + \frac{1}{128} (525 \cos \theta - 3150 \cos^3 \theta + 3465 \cos^5 \theta) \frac{a^6}{r^7} \right. \\ \left. - \dots \dots \right\} \dots \dots \dots (2)$$

Each of these expressions consists of a series of terms in ascending powers of $\frac{a}{r}$ which will be converging.

We shall now seek to find X and Y for a galvanic current traversing a wire coiled into the form of a hollow cylinder, of which the internal radius is b , the external radius $b+c$, and of which the length is $2f$. We shall suppose the individual turns of the wire to lie so close as that each may be regarded as an exact circle.



Let AB be the axis of the coil, so that A and B are the centres of its two faces; then $AB=2f$. Let O be the middle point of AB. Let P be the attracted point, PM its perpendicular distance p from AB. Let $PAM=\alpha$, $PBM=\beta$.

Let C be the centre of any turn of the wire regarded as a circle of radius a , $CP=r$, $PCM=\theta$, $OC=x$; then it is readily seen that for the whole cylindrical bobbin the forces X, Y are given by

$$\frac{X}{\mu} = \int_{-f}^{+f} \int_b^{b+c} L dx da,$$

$$\frac{Y}{\mu} = \int_{-f}^{+f} \int_b^{b+c} M dx da,$$

where L and M stand for the expressions on the right-hand side of (1) and (2) respectively, and where μ depends on the strength of the current.

To perform the integrations for the length of the bobbin in these expressions, we have the formulæ

$$\begin{aligned} p &= r \cdot \sin \theta, \\ \delta x \cdot \sin \theta &= -r \cdot \delta \theta; \\ \therefore \delta x &= \frac{-p \delta \theta}{\sin^2 \theta}, \end{aligned}$$

and

$$r = \frac{p}{\sin \theta}.$$

Making these substitutions for δx and r , the integrals with respect to x become integrals with respect to θ , which can be easily evaluated by a continued application of the method of integration by parts, the limits being from $\theta = \alpha$ to $\theta = \beta$. If we then integrate the result thus obtained with respect to a , from the limit b to the limit $b+c$, we finally obtain

$$\begin{aligned} \frac{X}{\mu} &= \frac{\overline{b+c}^3 - b^3}{6p^2} \{ -(\cos \beta - \cos \alpha) + (\cos^3 \beta - \cos^3 \alpha) \} \\ &\quad + \frac{\overline{b+c}^5 - b^5}{80p^4} \{ -9(\cos \beta - \cos \alpha) + 33(\cos^3 \beta - \cos^3 \alpha) \\ &\quad \quad - 39(\cos^5 \beta - \cos^5 \alpha) + 15(\cos^7 \beta - \cos^7 \alpha) \} \\ &\quad + \frac{\overline{b+c}^7 - b^7}{896p^6} \{ -75(\cos \beta - \cos \alpha) + 575(\cos^3 \beta - \cos^3 \alpha) \\ &\quad \quad - 1590(\cos^5 \beta - \cos^5 \alpha) + 2070(\cos^7 \beta - \cos^7 \alpha) \\ &\quad \quad - 1295(\cos^9 \beta - \cos^9 \alpha) + 315(\cos^{11} \beta - \cos^{11} \alpha) \} \\ &\quad + \dots\dots\dots, \\ \frac{Y}{\mu} &= \frac{\overline{b+c}^3 - b^3}{6p^2} \{ +(\sin^3 \beta - \sin^3 \alpha) \} \\ &\quad + \frac{\overline{b+c}^5 - b^5}{80p^4} \{ -12(\sin^3 \beta - \sin^3 \alpha) + 15(\sin^7 \beta - \sin^7 \alpha) \} \\ &\quad + \frac{\overline{b+c}^7 - b^7}{896p^6} \{ +120(\sin^7 \beta - \sin^7 \alpha) - 420(\sin^9 \beta - \sin^9 \alpha) \\ &\quad \quad + 315(\sin^{11} \beta - \sin^{11} \alpha) \} \\ &\quad + \dots\dots\dots \end{aligned}$$

These expressions for X and Y will be converging for all points situated at a greater distance than $b+c$ from any point of the axis AB , inasmuch as they are composed by adding together corresponding terms of series which are then all convergent. Among other points, these expressions hold for such as are situated on the axis external to the bobbin, and not nearer A or B than by the distance $(b+c)$. For such points, however, the expressions become illusory, assuming the form $\frac{0}{0}$. They may, how-

ever, be evaluated by the methods for the evaluation of vanishing fractions. Y is clearly zero. X may be more readily obtained directly from the expression for U . From that expression we find that for a single circular current the attraction on such points is

$$X = 2\pi k \left\{ + \frac{a^2}{r^3} - \frac{3}{2} \frac{a^4}{r^5} + \frac{15}{8} \frac{a^6}{r^7} - \dots \right\}.$$

Hence, in the case of a bobbin, if x be the distance of the attracted point from O , the middle point of the axis of the bobbin, we have

$$\begin{aligned} \frac{X}{\mu} &= \int_{x+f}^{x-f} \int_b^{b+c} dr da \left(+ \frac{a^2}{r^3} - \frac{3}{2} \frac{a^4}{r^5} + \frac{15}{8} \frac{a^6}{r^7} - \dots \right) \\ &= - \frac{\overline{b+c}^3 - b^3}{6(x^2 - f^2)^2} (\overline{x+f^3} - \overline{x-f^3}) \\ &\quad + 3 \frac{\overline{b+c}^5 - b^5}{40(x^2 - f^2)^4} (\overline{x+f^4} - \overline{x-f^4}) \\ &\quad - 5 \frac{\overline{b+c}^7 - b^7}{112(x^2 - f^2)^6} (\overline{x+f^6} - \overline{x-f^6}) \\ &\quad + \dots, \end{aligned}$$

which gives X for points situated on the axis for which x is not less than $(b+c+f)$.

December 12, 1872.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

Announcement was made from the Chair that the President had appointed Dr. Sharpey a Vice-President.

The Presents received were laid on the Table, and thanks ordered for them.

The following communications were read :—

- I. "A Contribution to the Knowledge of *Hæmoglobin*." By E. RAY LANKESTER, M.A. Oxon., Fellow of Exeter College, and Radcliffe Travelling Fellow of the University. Communicated by Prof. HUXLEY. Received July 29, 1872.

The fact that exceedingly small quantities of *Hæmoglobin* can be detected with great facility by means of the microspectroscope, has rendered it possible to trace the distribution of this important body among organisms of various classes, and by comparing its absence from certain animals or

tissues and its presence in others, with the accompanying conditions and activities belonging to the particular organisms, to arrive at some data bearing upon its physiological significance. Such a comparative method has already yielded results as to the activities of many organs and tissues, and is in all probability destined in the future, when applied to the more minute problems of the functions of structures and tissue-components, to give new impulse to the science of physiology.

Before communicating some newly observed facts with regard to the occurrence of *Hæmoglobin* in animal organisms, I may briefly state what is the present condition of the subject.

After Hoppe Seyler* and Stokes† had shown that the red colouring-matter of the blood of *Vertebrata* could be recognized by its peculiar absorption-spectrum, Kuhne‡ discovered that the same colouring-matter, the oxygen-carrying properties of which were known from other researches, was diffused in the voluntary muscular tissue of mammals, and imparted to them their red tint. Rollett§ then obtained from the red vascular fluid of the earthworm crystals which were identical with those of *Hæmoglobin*, and Nawrocki|| at the same time as myself¶ confirmed the supposition that *Hæmoglobin* is the cause of the red coloration of the blood of *Lumbricus*, by careful spectroscopic observation of the fluid and the derivatives yielded by it (*Hæmatin*). I also established by spectroscopic analysis** the existence of *Hæmoglobin* in the blood of the mollusk *Planorbis*, in that of the larva of the insect *Cheironomus*, in that of the Crustaceans *Cheirocephalus* and *Daphnia*, and in the vascular fluids of the marine Annelids *Eunice*, *Nereis*, *Terebella*, and others. I found also that in the Annelids of the family "Chlorémiens" of Audouin and Edwards, as well as in some species of *Sabella*, the *Hæmoglobin* was replaced by a body having similar properties, giving a dark red colour to the vascular fluid when seen in sufficient thickness, and a bright green in thinner layers. This body gave a very sharply marked and characteristic pair of absorption-bands in the oxidized condition, which were changed to a single one in the reduced condition, as in the case of *Hæmoglobin*, the bands, however, having a relative intensity and a position altogether differing from those of *Hæmoglobin*. By the action of cyanide of potas-

* Hoppe Seyler, "Ueber die chemischen u. optischen Eigenschaften des Blut-farbstoffs," *Archiv f. pathol. Anat. u. Physiol.* 1862, Bd. xxiii.

† Stokes, "On the Reduction and Oxidation of the Colouring-matter of the Blood," *Proc. Royal Society*, 1864.

‡ Kuhne, "Ueber den Farbstoff der Muskeln," *Archiv f. Pathol. Anat. u. Physiol.* Bd. xxxiii.

§ Rollett, "Zur Kenntniss der Verbreitung des Hämätins," *Sitzungsber. d. k. Ak. d. Wiss. z. Wien*, Bd. xlv.

|| Nawrocki, *Centralblatt für die medic. Wissenschaften*, 1867, p. 196.

¶ Lankester, "Observations with the Spectroscope," *Journal of Anatomy and Physiology*, 1867, p. 114.

** "Spectroscopic examination of certain Animal Substances," *ibid.* 1869, p. 119.

sium, followed by that of a reducing agent (sulphide of ammonium), this body, to which the name Chlorocruorin was given, furnished two absorption-bands identical with those exhibited by Hæmoglobin when similarly treated. Last year I found that the red colour of the pharyngeal muscles of the Gasteropods *Lymnæus* and *Paludina** was due to the presence of Hæmoglobin diffused in the muscular tissue; and at the Meeting of the British Association at Edinburgh I demonstrated its occurrence in the pharyngeal muscular mass of *Littorina*, where it is in sufficient quantity to give a very intense blood-red colour. The interest of this fact consists in this, that in no other part, not even in the blood of these mollusks, does Hæmoglobin occur; hence the doubts which Brozeit† had attempted to throw upon Kuhne's conclusions with regard to the red colour of mammalian voluntary muscular tissue were rendered of less significance than before. It was also mentioned that whilst Hæmoglobin is absent from the unstriped muscular tissue of mammalia generally, it occurs in that tissue in the rectum of man, and probably of other mammals.

Some observations made during the past winter at Naples will tend to enlarge the basis of facts sketched above, upon which I have ventured to make some generalizations with regard to the mode of occurrence of Hæmoglobin in the animal economy.

1. *Occurrence of Hæmoglobin in corpuscles in the perivisceral fluid of the Annelids Glycera and Capitella, and of the Gephyrean Phoronis.*—Many of the marine Chaetopodous Annelids are devoid of that closed vascular system which in others contains a fluid impregnated with Hæmoglobin or Chlorocruorin. *Glycera* is one of these anangian forms; but, as an exception to the general rule, has peculiar corpuscles in its perivisceral fluid which are of a red colour, as was observed by De Quatrefages and confirmed by Claparède. These corpuscles I find, when submitted to the microspectroscope, give an absorption-spectrum identical with that of Hæmoglobin, as proved by the superposition of the spectra. The Hæmoglobin of these corpuscles was readily obtained in both the oxidized and the reduced conditions. The corpuscles (Pl. I. figs. 1, 2, & 3) which contain the Hæmoglobin are round, somewhat flattened, cheese-shaped bodies $\frac{1}{1800}$ of an inch in diameter, possessing a very delicate wall, which often becomes irregularly puckered by increased density of the fluid in which the corpuscles float, and a scarcely visible nucleus about $\frac{1}{2000}$ of an inch in diameter eccentrically placed, which is brought into sharp definition by the action of weak acids, and exhibits granulation of its contents. These corpuscles exhibit a tendency to run into rouleaux, and form aggregations similar to those known in the case of mammalian and other vertebrate red corpuscles. They are by no means so numerous in the perivisceral fluid as are the red corpuscles in vertebrate blood, and are accompanied

* "Verbreitung des Hæmoglobins," Archiv f. ges. Physiol. 1871.

† "Bestimmung der Blutmenge im Körper," Archiv f. ges. Physiol. Bonn, 1870, p. 353.

by amœboid corpuscles, which are colourless, and by the genital products. When a *Glycera* is irritated so as to cause it frequently to extrude its large pharyngeal tube and jaws, a red line is noticed running along the whole of the ventral surface. This appears to be caused by the pressure of the fluid containing the red corpuscles into the cavity of the outer sheath of the nerve-chord, and may have some functional importance. The corpuscles enter this sheath at its anterior termination*.

2. *Occurrence of Hæmoglobin in the vascular fluid of Turbellarians.*—Quatrefages observed that the fluid in the vessels of the Nemertean worm *Polia sanguirubra* was coloured red. In a species examined at Naples, I have found that this colour is due to Hæmoglobin diffused in the liquid. In by far the majority of cases the vascular fluid of the Turbellaria is colourless. My friend Mr. H. N. Moseley last year observed Hæmoglobin with the spectroscope in a small Planarian at Suez.

3. *Occurrence of Hæmoglobin in corpuscles in the blood of a Lamelli-branchiate Mollusk.*—*Solen legumen*, which is known to the Neapolitan fishermen as “*Canolicca femina*,” has a reddish tint; and this was found to be due to the red colour of the blood, which could be seen very beautifully, like a natural injection, in the blood-sinuses and vessels of the mollusk. When one of the shell-valves was partially removed so as to obtain a clearer view of the underlying parts, the red-coloured fluid could be seen passing from one of the large sinuses to another, as the animal expanded and retracted its long worm-like foot. I would particularly draw attention to the fact that though, when thus irritated, an increased amount of liquid appeared to be exuded from the mollusk, yet this liquid was entirely colourless and devoid of corpuscles—a fact which must qualify existing notions as to the escape of the organized blood-fluid of Mollusca, based upon the well-ascertained existence of apertures communicating with the exterior in their vascular system. If the liquor sanguinis escapes, it is clear that at any rate the corpuscles, in which, as will be mentioned, the red colouring-matter resides, do not. A very slight injury to the tissue of the mantle is sufficient to cause an escape of the red-coloured fluid; and this when placed under the microscope is found to consist of a plasma in which float abundant oval, sharply contoured cells of a red colour (figs. 4 & 5). The red colour was definitely proved to be due to Hæmoglobin by examination with the microspectroscope, giving the two bands identical with those of O, Hb from human blood, and furnishing

* To the case here recorded, of Hæmoglobin occurring in corpuscles among Vermees, I can now add that of the annelid *Capitella*, the corpuscles of which I found similar to those of *Glycera*, and further, the remarkable Gephyrean *Phoronis hippocrepia* (fig. 6). In the annelids *Cirratulus* and *Ophelia*, corpuscles occur in the closed vascular system, which contains a liquid in which Hæmoglobin is diffused. Such a vascular fluid is common among the Chaetopoda; but these are the only cases known in which it contains corpuscles. On examining these corpuscles, which are very small in number, I find them to be not special Hæmoglobin-bearing corpuscles, but small amœboid particles which are impregnated with Hb, just as the surrounding fluid is.

also the single band of reduced Hb when treated with reducing agents. The corpuscles measure about $\frac{1}{3200}$ of an inch in length and $\frac{1}{3750}$ in breadth. They are by far the most abundant, but not the only corpuscles in the liquid. Colourless amœboid corpuscles are also present. Acetic acid brings out a clearly emarginated nucleus in the red corpuscles. Acetate of rosaniline, both with these corpuscles and the red corpuscles of *Glycera* and of *Phoronis*, stains the nucleus intensely, and gives one or two eccentrically placed maculæ, as was observed by Dr. Roberts in human and vertebrate blood generally. I have not obtained these maculæ in corpuscles devoid of Hæmoglobin; and since, in the cases of *Glycera*, of *Phoronis*, and *Solen legumen*, where this body is present, as in the vertebrate red corpuscles, the Robertsian macula is obtained, there is some ground for believing that the substance which is thus rendered evident by coloration with magenta is connected with the Hæmoglobin, being either a part of its decomposition-product, due to the action of the rosaniline, or a necessary concomitant of it in the blood-corpuscle. Since the other species of *Solen* accompanying *Solen legumen* in the Gulf of Naples have colourless blood, I was anxious to see whether the corpuscles which bear the Hæmoglobin in that species are represented in any way in the others. I therefore examined the blood of *Solen ensis*. I found but few corpuscles present; and these were all perfectly colourless, and exhibited very active amœboid movements, throwing out processes in various directions with great rapidity, and running together into adherent masses on the glass slip; whilst under the microscope magenta brought a large nucleus in them, with brilliant staining, but gave no trace of an eccentric macula, such as that which characterizes Hæmoglobin-bearing corpuscles under similar treatment.

4. *Absence of Hæmoglobin from the blood of the fish Leptocephalus, with presence of corpuscles corresponding to the red corpuscles.*—The beautiful little fish *Leptocephalus* is, with the exception of the black-pigmented eyes, perfectly colourless and glass-like. I am not aware of traces of red colour in its blood having been carefully sought for; but there certainly are none. The blood-vessels possess no tint whatever, and the gills, moreover, are perfectly free from colour. Notwithstanding this absence of Hæmoglobin, the blood of *Leptocephalus* presents, as observed by Kölliker, both the colourless amœboid corpuscles of vertebrate blood and elliptic nucleated corpuscles, also colourless, exactly comparable to those which bear the Hæmoglobin in other fish. This fact is remarkable when put side by side with the observation that the perivisceral fluid of Annelids generally presents no corpuscles comparable to those which bear Hæmoglobin in *Glycera*, *Capitella*, and *Phoronis*. The ordinary amœboid corpuscles are present in both series; and in the latter the Hæmoglobin-bearing corpuscles appear as a special addition.

In *Amphioxus* I may mention that I have not succeeded, after trials, in obtaining spectroscopic evidence of Hæmoglobin, though Wilhelm

Müller, of Jena, states that this vertebrate has corpuscles of a pale red tint. If *Amphioxus* should prove not to, it is yet certain that *Leptocephalus* does form an exception to Prof. Preyer's recent statement in his valuable monograph, 'Die Blutkrystalle,' that Hæmoglobin is found "bei allen Vertebraten."

The statements in the present notice will be found to extend considerably the area of distribution of Hæmoglobin, as given in Prof. Preyer's work, which is the latest (1871) on the subject.

5. *Presence of Hæmoglobin in the tissue of the chain of nerve-ganglia of Aphrodite aculeata*.—The chain of nerve-ganglia of the Annelid *Aphrodite aculeata* possesses a bright crimson tint. This colour is particularly marked in the ganglia themselves, and is most intense in the supracæso-phageal ganglion, which has as intense a colour as a drop of fresh human blood. When examined with the microscope, the colour is found to impregnate the nerve-tissue itself, and not to be held in any liquid bathing the tissue.

The vessels of *A. aculeata*, which are not represented in closely allied species (which are anangian according to the researches of Claparède), contain a pale lemon-coloured fluid, which gives no bands with the spectroscopy, and is certainly free from Hæmoglobin.

The perivisceral fluid is colourless, and also the muscular tissue throughout the animal, with the single exception of the great muscular pharynx. This has a pale pink tint; and it and the nerve-chain, when examined with the microspectroscope, gave abundant evidence of the presence of Hæmoglobin. The quantity in the pharyngeal muscular tissue is very small indeed; but, on the contrary, excessive in the nerve-ganglia, corresponding with their brilliant crimson colour.

I may mention here that the orange or yellow colour of the nerve-ganglia of many Mollusca is not due to Hæmoglobin.

6. *Presence of Hæmoglobin in the muscles of the dorsal fin of Hippocampus*.—The abundance of the little fish *Hippocampus* in the Gulf of Naples has enabled me to confirm an observation made three years ago on specimens which died in the aquarium in Regent's Park. Whilst the other locomotive muscles of this and other fish are quite free from colour, the muscles attached to the base of the dorsal fin are seen to have a pale red colour; and on examination with the spectroscopy this colour is found to be due to Hæmoglobin. Microscopic examination proves the colouring-matter to be diffused in the muscular tissue, and not to be due to a special vascularity of the part in question. Probably other specially active muscles of other fishes may be found to be in the same case as those of the dorsal fin of *Hippocampus*; but I have not made any extended observations on this point.

The deep red coloration of the heart in Vertebrata generally is readily proved by the spectroscopy to be due to the presence of Hæmoglobin; but owing to the great vascularity of the tissue in some cases, or to its

cavernous structure in others, and its consequent close association with the *Hæmoglobin* of the blood, this does not seem remarkable. I have not attempted thoroughly to remove the blood from the heart by injection of salt-solution, as was done by Kuhne with the voluntary muscular tissue of Mammals.

The facts ascertained as to the distribution of *Hæmoglobin* may now be summarized as follows:—

1. In special corpuscles.

a. In the blood of all vertebrates, excepting *Leptocephalus* and *Amphioxus* (?).

b. In the perivisceral fluid of some species of the Vermian genera *Glycera*, *Capitella*, and *Phoronis*.

c. In the blood of the Lamellibranchiate Mollusk *Solen legumen*.

2. Diffused in a vascular or ambient liquid.

a. In the peculiar vascular system of the Chaetopodous Annelids very generally, but with apparently arbitrary exceptions.

b. In the vascular system (which represents a reduced perivisceral cavity) of certain leeches, but not of all (*Nephelis*, *Hirudo*).

c. In the vascular system of certain Turbellarians as an exception (*Polia*).

d. In a special vascular system (distinct from the general blood-system) of a marine parasitic Crustacean (undescribed) observed by Professor Edouard van Beneden.

e. In the general blood-system of the larva of the Dipterous insect *Cheironomus*.

f. In the general blood-system of the pulmonate mollusk *Planorbis*.

g. In the general blood-system of the Crustaceans *Daphnia* and *Cheirocephalus*.

3. Diffused in the substance of muscular tissue.

a. In the voluntary muscles generally of Mammalia, and probably of birds, and in some muscles of reptiles.

b. In the muscles of the dorsal fin of the fish *Hippocampus*, being generally absent from the voluntary muscular tissue of fish.

c. In the muscular tissue of the heart of Vertebrata generally.

d. In the unstriped muscular tissue of the rectum of man, being absent from the unstriped muscular tissue of the alimentary canal generally.

e. In the muscles of the pharynx and odontophor of Gasteropodous Mollusks (observed in *Lymnæus*, *Paludina*, *Littorina*, *Patella*, *Chiton*, *Aplysia*), and of the pharyngeal gizzard of *Aplysia*, being entirely absent from the rest of the muscular and other tissues and the blood of these mollusks. See as to *Planorbis* above (2 f).

f. In the muscular tissue of the great pharyngeal tube of *Aphro-*

dite aculeata, being absent from the muscular tissue and from the blood in this animal, and absent from the muscular tissue generally in all other Annelids as far as yet examined.

4. Diffused in the substance of nervous tissue.

a. In the chain of nerve-ganglia of *Aphrodite aculeata*.

The significance of these observations depends to a large extent on the negative results given by very numerous observations not recorded here. I have taken every opportunity, during some years past, of examining coloured animal matters with the spectroscope, and especially where there could be a suspicion of the presence of Hæmoglobin*. Thus, where the absence of Hæmoglobin is generally stated above, it must be understood that examination has been made for it in such cases as have been accessible. I have found that many cases of red coloration of a tissue or liquid, which might be supposed to be due to Hæmoglobin, are certainly not so, such red-coloured matter failing to give the characteristic bands of that body, and, as a rule, giving no detached characteristic bands. Such are the red pigments occurring in the blood-corpuscles of *Sipunculus*, in the tissues of many Annelids, in Echinodermata, in compound Tunicata, surrounding the intestine of *Salpa*, in the foot and mantle of many Mollusca, also in their nerve-ganglia and other parts, in the chromatophores of Cephalopoda, in certain Infusoria. On the other hand, among coloured bodies not suggesting Hæmoglobin, I have found an equally large number devoid of characteristic spectra, but some few which exhibit the remarkable phenomenon of detached definite bands of absorption, which enables them to be certainly characterized and recorded. Such are:—a chlorophyll-like body occurring in *Spongilla*, in *Hydra viridis*, and in *Mesostomum viride*; Chlorocruorin, which takes the place of Hæmoglobin in the vascular fluid of the Chlorémiens and some species of *Sabella*; Stentorin, giving the intense blue colour to the Infusorian *Stentor coeruleus*, and possessing a very marked and peculiar pair of absorption-bands. With one single exception, it appears, from the examination of a great number of cases, both among Vertebrates and Invertebrates, that coloured bodies which may be supposed to be purely pigmentary in their function do not give detached absorption-bands. The exception is the red colouring-matter named Turacin by Professor Church, discovered by him in the feathers of birds of the family Musophagidæ, which has other properties quite unusual in pigmentary bodies. In an examination of a large number of birds' feathers, red, yellow, blue, and green, I failed to obtain detached absorption-bands, as

* I may state that I have not hitherto made any observations on the colouring-matters of the biliary secretion in Invertebrata and the lower Vertebrates, excepting in their fresh condition. The use of the spectroscope, combined with chemical reagents, would no doubt lead to interesting results in that field, since a variety of substances giving characteristic absorption-spectra have been obtained from the manipulation of mammalian bile-pigment.

also in the scales of fish and in the skin and hair of mammals, and in the pigments of many Crustaceans, Annelids, Insects, Tunicates, and Sponges*.

From a consideration of the facts stated above with regard to the mode of occurrence and distribution of *Hæmoglobin* in animal organisms, the following general statements may be made, which are in accordance with the now thorough establishment, by chemical investigation, of its peculiar oxygen-carrying property.

Hæmoglobin is irregularly distributed throughout the animal kingdom, being absent entirely only in the lowest groups†. It may be present in all the representatives of a large group, with but one or two exceptions, or it may be present in one only out of the numerous members of such a group; or, again, it may be present in one and absent in another species of the same genus. It may occur in corpuscles in the blood, or diffused in the liquor sanguinis, or in the muscular tissue, or in the nerve-tissue. The same apparent capriciousness characterizes its occurrence in tissues as in specific forms. It may be present in one small group of muscles and absent from all the rest of the tissues of the body, or it may occur in one part only of a tissue, histologically identical throughout its distribution in the organism. The apparently arbitrary character of this distribution is to be explained (though only partially) by a reference to the chemical activity of *Hæmoglobin*. Wherever increased facilities for oxidation are requisite, *Hæmoglobin* may make its appearance in response; where such facilities can be dispensed with, or are otherwise supplied, *Hæmoglobin* may cease to be developed.

The Vertebrata and the Annelida possess a blood containing *Hæmoglobin* in correlation with their greater activity as contrasted with the Mollusca, which do not possess such blood. The actively burrowing *Solen legumen* alone amongst Lamellibranchiate Mollusks, and amongst Gasteropods only *Planorbis*, respiring the air of stagnant marshes, possess blood containing *Hæmoglobin*. In the former the activity, in the latter the deficiency of respirable gases are correlated with the exceptional development of *Hæmoglobin*. But we cannot as yet offer an explanation of the absence of *Hæmoglobin* from the closely allied species of *Solen*, and from the *Lymnæi* which accompany *Planorbis*. The Crustaceans *Cheirocephalus* and *Daphnia*, and the larva of *Cheironomus*, possessing, as exceptions in their classes, *Hæmoglobin* in their blood, inhabit stations where the amount of accessible oxygen must be small (that is to say, stagnant ponds), the last living in putrescent mud; whilst the possession

* See Journal of Anatomy and Physiology, 1869-70, p. 119.

† [Note. Dec. 24th, 1872.]—It is perhaps of some significance that *Hæmoglobin* has only been found in that great group of the animal kingdom which in the course of its development gives rise to a middle layer of blastodermic cells or mesoderm, and in examples from nearly every great branch of this stem.

of abundant Hæmoglobin in its vascular fluid may be supposed to be one of the chief properties which enables the oligochaet Annelid *Tubifex* to hold its ground in the foul, and therefore much deoxygenated, water of the Thames at London.

The known chemical properties of Hæmoglobin furnish a more complete explanation of its peculiar distribution in tissues. That it should occur in a circulating fluid, which is the medium of respiration, is obviously related to those properties. Its occurrence in the voluntary muscles of the most active of Vertebrata, and in the most active muscles of some others (as in the case of the dorsal-fin muscles of *Hippocampus*), is equally so; so also its occurrence in the most powerfully acting part of the intestinal muscles, those of the rectum, and in the only rapidly and constantly acting muscles of the Gasteropoda, namely those used in biting and rasping.

To connect its occurrence in the nervous chain of *Aphrodite aculeata* with its properties is more difficult, since we have no knowledge that this Annelid is remarkable for nervous energy. The large bulk of the animal in proportion to the size of the nervous system, and the deficient respiration, indicated by the very slightly developed vascular system and the total absence of Hæmoglobin from the fluids of the worm, may be a reason for the endowment of the nervous centre which has to control such a large and complicated organism with a special facility for appropriating what little oxygen may come in its way.

The complete absence of Hæmoglobin from *Leptocephalus* is an example of the submission of an auxiliary, but not an essential, structural attribute to an all-powerful necessity—that of transparency. The absence of Hæmoglobin from the transparent Annelid *Alciops* may be similarly correlated.

From what has been stated above as to the Hæmoglobin-bearing corpuscles of *Glycera*, *Solen*, and the Vertebrata, it appears that when Hæmoglobin is present in the blood in *corpuscles*, these corpuscles are of a peculiar character, and are specially related to the presence of the Hæmoglobin. When that is absent, other things remaining the same (as with the blood of *Solen ensis* and the perivisceral fluid of most Annelids), the peculiar corpuscles are absent. Such things as colourless corpuscles, representative of the Hæmoglobin-bearing corpuscles, do, however, appear to exist in the case of the fish *Leptocephalus*. In connexion with the relation of the colourless corpuscles of vertebrate blood to the red corpuscles, and of the corpuscles of the vascular fluids of Invertebrata to one another and to those of Vertebrates, these facts seem to be important: the colourless corpuscles in one case are only comparable to the colourless in another; the red corpuscles are something apart, which may or may not be superadded*.

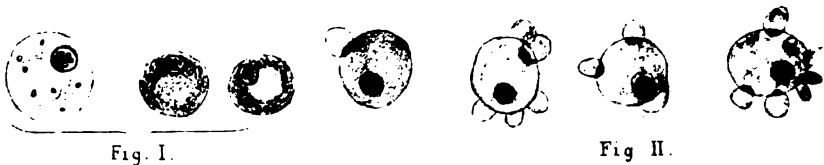
* [Note. Dec. 24th, 1872.]—The two kinds of corpuscles may be definitely distinguished from one another as *leucocytes* and *pneumocytes*.

The corpuscles of the perivisceral fluid of the Gephyrean *Sipunculus nudus*, which is abundant in the Gulf of Naples, present some facts which are interesting in relation to the occurrence of *Hæmoglobin*; and I may therefore draw attention to them before concluding this paper. The fluid which is contained in the perivisceral cavity of this worm is, as is well known, of a pale madder-red colour. It contains a remarkable abundance and variety of corpuscles, the most numerous of which are thick circular disks, varying in diameter from $\frac{1}{8000}$ to $\frac{1}{3000}$ of an inch; and in these, and these only, the pink colour resides (fig. 7). These pink corpuscles consist of a clear homogeneous substance, of high refringent power, in which are scattered three or four bright granules and a small nucleus, which is rendered obvious by the action of acetic acid. Rosaniline stains this nucleus, but does not usually give any other maculæ, such as are to be observed when it is added to *Hæmoglobin*-containing corpuscles*. Dr. Alexander Brandt, in a recent memoir, very rightly insists on the similarity between these pink corpuscles of *Sipunculus* and the red corpuscles of the blood of *Vertebrata*: they are something quite distinct from the amœboid corpuscles found in the fluid corresponding to blood in nearly all *Invertebrata*, and are to be compared to the red corpuscles of *Glycera*, *Solen*, and *Vertebrates*. The amœboid corpuscles are otherwise represented in the perivisceral fluid of *Sipunculus* by numerous active amœboid cells. Dr. Brandt, naturally enough, regarded the pink colour of these corpuscles as favouring their assimilation to vertebrate red corpuscles. The colour *en masse* is, however, obviously different from that of dilute *Hæmoglobin*; and I was not therefore surprised to find that it did not give the absorption-spectrum of that body. This pink colouring-matter is soluble in water. When a little fresh water is added to some of the perivisceral fluid in a tube, it takes up all the colour, whilst the corpuscles sink in a colourless condition to the bottom. No detached bands of absorption of any kind were given by the colouring-matter thus obtained; a slight acidulation with acetic acid was sufficient to destroy the colour. Ammonia had the same action, also ether and alcohol.

Though this pink substance is thus devoid of the spectral properties which characterize *Hæmoglobin* and *Chlorocruorin*, it does not seem improbable that it is a body analogous to them in other properties, since the corpuscles in which it resides can only be compared to the respiratory or oxygen-carrying corpuscles occurring in the blood of *Vertebrates* and the four *Invertebrates* noticed in this paper. Moreover this pink colouring-matter occurs in other parts of the organism of *Sipunculus*, namely, diffused in the substance of a remarkable tissue which runs along the wall of the intestine, forming a red streak, which has sometimes been taken for a blood-vessel, and also in the peculiar cellular tissue which surrounds the true nerve-tissue of the nerve-chord.

* On one occasion out of many I obtained an appearance of the kind; and hence further observation on this point is necessary.

GLYCERA.



GLYCERA.

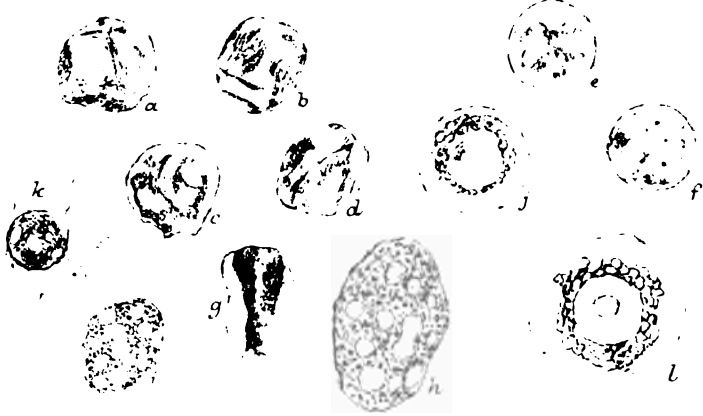


Fig. III.

SOLEN



Fig. IV.

SOLEN



Fig. V.

PHORONIS

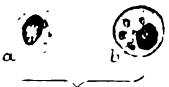


Fig. VI.

SIPUNCULUS.



Fig. VII.

The occurrence of colourless corpuscles in *Leptocephalus* identical in form and character with the Hæmoglobin-bearing corpuscles of the blood of other fish, and the apparently capricious distribution of Hæmoglobin among Invertebrata, together with the existence of the green oxygen-carrier Chlorocruorin and the pink colouring-matter of the corpuscles of *Sipunculus nudus*, suggest the hypothesis of the existence of various bodies not necessarily red, possibly colourless, which act the same physiological part in relation to oxygen as does Hæmoglobin.

DESCRIPTION OF PLATE I.

- Fig. 1. *Glycera*. Red corpuscles acted on by acetic acid.
 Fig. 2. *Glycera*. Red corpuscles acted on by magenta.
 Fig. 3. *Glycera*. *a* to *f*, normal red corpuscles; *g*, seen laterally; *h*, *i*, testicular cell-masses; *k*, leucocyte; *j*, *l*, ova.
 Fig. 4. *Solen legumen*. *a*, *b*, *c*, normal red corpuscles; *d*, *e*, leucocytes of *Solen ensis* identical with leucocytes of *S. legumen*, with nucleus stained by magenta.
 Fig. 5. *Solen legumen*. *a*, *b*, *c*, red corpuscles acted on by dilute acetic acid; *d*, *e*, *f*, *g* ditto acted on by magenta.
 Fig. 6. *Phoronis hippocrepia*. Red corpuscles acted on by dilute acetic acid.
 Fig. 7. *Sipunculus nudus*. *a* to *g*, normal pink-coloured corpuscles of the perivisceral fluid; *b* contains a crystal; *h*, leucocyte from the same fluid; *i*, *j*, pink corpuscles (pneumocytes) acted on by dilute acetic acid.

II. "On the Structural Composition of Urinary Calculi." By
 H. VANDYKE CARTER, M.D. Lond. Communicated by L. S.
 BEALE, M.D., F.R.S. Received October 11, 1872.

(Abstract.)

Having occasion during his late residence in Western India to remove numerous urinary calculi from persons belonging to the indigenous population, the author was enabled to pursue an inquiry into the character of these concretions which he had begun in the year 1859 by the chemical analysis of a large number of such calculi; it remained then to make use of the microscope as a means of investigation; and since no record of a similar systematic inquiry to that now undertaken has been published, so far as known to the author, it was thought desirable that this hiatus in medical literature should, however imperfectly, be forthwith filled up.

The plan adopted was to submit minute fragments taken from the real or apparent nuclei, and from succeeding layers and crust, of the calculi examined to the scrutiny of average optical powers, the highest available magnifying about 300 diameters. Distilled water was the ordinary medium employed; and in all cases chemical tests were conjointly used for the purposes of detection or confirmation. After sufficient practice, it became apparent that the microscopic analysis of calculi, thus carried

out, is not only valuable, but that it is even more delicate than the simply chemical method; by it will be seen more clearly than before that no urinary deposit long retains an unmixed character, and besides will be gained a more accurate conception of the true character and structure of these hurtful concretions.

Particular attention has been given to the nature of their nuclei or first-formed ingredients, for it was obviously desirable to ascertain the material and mode of commencement of calculi; and the author regards it as not the least interesting of his results to have determined the very frequent presence of urates in a globular form at their very earliest beginnings. Stones removed from adults as well as from children may possess a nucleus thus formed, and to such origin may succeed all varieties of calculous deposit. The next most common ingredient of the nucleus is the oxalate of lime, and this, too, in a form not generally recognized, viz. as large rhombic crystals; frequently, however, octahedral, oval, and dumb-bell forms are seen, as well as spheroidal bodies originating from the latter; these last-named structures are very characteristic. The nucleus of a calculus was comparatively seldom found to consist of uric-acid crystals.

Next in importance to the observation that globular and granular urates very frequently compose the nucleus of all varieties of calculi, and have commonly associated with them the oxalate of lime, is the evidence elicited of the operation of known physical influences in determining the form which these salts assume in connexion with stone. Reference is made to the modifying influence of a colloid medium upon the process of crystallization (as so admirably illustrated in Mr. Rainey's well-known researches); and it is here found that both urates and oxalates may present the various stages of globular particles, complex globules, or spheroids, either separate or blended, and laminae of varying thickness: there are evident signs, too, that these structures undergo, at times, more or less disintegration, which may be followed by a rearrangement of their molecules; and more especially does the calcic oxalate seem disposed to undergo these changes, the resulting forms being highly characteristic. Neither uric acid nor the phosphates were found otherwise than in a simpler crystalline form; the former, however, when liquor potassæ is added, may in combination be noticed to pass into minute globular particles, the urate of potash being seen in the field of the microscope to assume some of the characters of ordinary urates; and phosphate of lime artificially produced was also observed in the shape of dumb-bells and spheres.

The author refers to the structure of Raphides, agates, some shells, &c., and concludes that urinary calculi should not be classed with ordinary concretions or mere mineral masses; it is not, however, evident that the animal basis of calculous matter, essential ingredient though it be, presents or retains a strictly cellular character.

Some details respecting the source and other characters of the calculi

submitted to examination are added, with a Table specifying the sex, age, &c. of patients, weight and composition of the specimens, &c.

A second Table shows an arrangement of the ascertained ingredients in a form which, if not complete, may yet be useful; in it the rarer substances belonging to urinary calculi are not included.

Next follows a description of each well-known ingredient as it was observed to occur:—*Uric acid* is here crystalline, but not in the shapes prevalent in urinary deposits; free crystals (1) are comparatively rare, and open columns (2), or else compact laminæ (3), are most frequent; amidst these the very general occurrence of simple oxalate-of-lime forms is mentioned. The *Urates* are present in calculi, as (1) granules, (2) acicular crystals, (3) globules, (4) laminæ: the relation of the two latter forms is described, and the apparent character of their animal basis. *Oxalate of lime* would seem to be the most prevalent of all ingredients, and it exists as (1) granules, if the particles were really shapeless, as they appeared to be; (2) crystals—octahedral (often of enormous size), ovoid, and dumb-bell shaped; (3) spheroids, or large composite globular particles of very characteristic appearance, and originating from the last-named form; (4) laminæ, also characteristic. Some remarks are added on crystals of the oxalate artificially produced. Certain *crystals*, styled *peculiar*, are described; they are frequently admixed with the urates, and, upon grounds specified in detail, are judged to be a form of the calcic oxalate. *Phosphate of lime*: free crystals of this salt have not been seen in calculi; and compact layers, authoritatively recognized, have varied in appearance either as the result of disintegration, or possibly in consequence of some difference in chemical composition—a point amongst others needing further investigation. Respecting the *triple phosphate* and the *fusible calculus*, details of their microscopic appearance are submitted in the communication under notice.

In conclusion, figures arranged in four Plates, with descriptions, are appended.

III. "Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun."—No. I. By J. NORMAN LOCKYER, F.R.S. Received November 6, 1872.

(Abstract.)

The author, after referring to the researches in which he has been engaged since January 1869 in conjunction with Dr. Frankland, refers to the evidence obtained by them as to the thickening and thinning of spectral lines by variations of pressure, and to the disappearance of certain lines when the method employed by them since 1869 is used. This method consists of throwing an image of the light-source to be examined on to the slit of the spectroscope.

It is pointed out that the phenomena observed are of the same nature as those already described by Stokes, W. A. Miller, Robinson, and Thalen, but that the application of this method enables them to be better studied, the metallic spectra being clearly separated from that of the gaseous medium through which the spark passes. Photographs of the spark, taken in air between zinc and cadmium and zinc and tin, accompany the paper, showing that when spectra of the vapours given off by electrodes are studied in this manner, the vapours close to the electrode give lines which disappear from the spectrum of the vapour at a greater distance from the electrode, so that there appear to be long and short lines in the spectrum.

The following elements have been mapped on this method:—Na, Li, Mg, Al, Mn, Co, Ni, Zn, Sr, Cd, Sn, Sb, Ba, and Pb, the lines being laid down from Thalen's maps, and the various characters and lengths of the lines shown.

In some cases the spectra of the metals, enclosed in tubes and subjected to a continually decreasing pressure, have been observed. In all these experiments the lines gradually disappear as the pressure is reduced, the *shortest lines disappearing first, and the longest lines remaining longest visible*.

Since it appeared that the purest and densest vapour alone gave the greatest number of lines, it became of interest to examine the spectra of compounds consisting of a metal combined with a non-metallic element. Experiments with chlorides are recorded. It was found in all cases that the difference between the spectrum of the chloride and the spectrum of the metal was that under the same spark-conditions all the short lines were obliterated. Changing the spark-conditions, the final result was that only the very longest lines in the spectrum of the metallic vapour remained. It was observed that in the case of elements with low atomic weights, combined with one equivalent of chlorine, the numbers of lines which remain in the chloride is large, 60 per cent., *e. g.*, in the case of Li, and 40 per cent. in the case of Na; while in the case of elements with greater atomic weights, combined with two equivalents of chlorine, a much smaller number of lines remain—8 per cent. in the case of barium, and 3 per cent. in the case of Pb.

The application of these observations to the solar spectrum, to elucidate which they were undertaken, is then given.

It is well known that all the known lines of the metallic elements on the solar atmosphere are not reversed. Mr. Lockyer states what Kirchhoff and Ångström have written on this subject, and what substances, according to each, exist in the solar atmosphere. He next announces the discovery that, with no exception whatever, *the lines which are reversed are the longest lines*. With this additional key he does not hesitate to add, on the strength of a small number of lines reversed, zinc and aluminium (and possibly strontium) to the last list of solar elements given by Thalen, who

rejected zinc from Kirchhoff's list, and agreed with him in rejecting aluminium. It need scarcely be added that these lines are in each case the longest lines in the spectrum of the metal.

The help which these determinations afford to the study of the various cyclical changes in the solar spectra is then referred to.

December 19, 1872.

Sir GEORGE BIDDELL AIRY, K.C.B., President, followed by Mr. BUSK, Vice-President, and Dr. SIBSON, Vice-President, in the Chair.

The following communications were read :—

- I. "Magnetical Observations in the Britannia and Conway Tubular Iron Bridges." By Sir GEORGE BIDDELL AIRY, K.C.B., P.R.S., Astronomer Royal. Received October 12, 1872.

(Abstract.)

The author states that he was first induced to make these experiments by consideration of the perpetual tremor to which the iron of these structures is subjected, and which appeared likely to have made them unusually sensitive to the production of induced and perhaps subpermanent magnetism. The experiments were actually conducted by Mr. James Carpenter (then Assistant at the Royal Observatory), with the friendly cooperation of Captain Tupman, R.M.A. Permission was given by the Directors of the London and North-Western Railway Company, and every possible assistance was given by the resident Officers of the Company, with a degree of zeal and liberality which cannot be too highly appreciated. The observations were made in the axis of each tube, a large step-ladder and stage adapted to the circumstances having been provided; the observations made were those of disturbed magnetic azimuth, disturbed time of vibration of a horizontal needle, and disturbed dip. The places of observation were :—in each line of the Britannia Bridge, a station on each of its five supporting towers and a station in the middle of each of the four sections of the continuous tube, and also stations in the prolongations of the axis of the bridge to a considerable distance in each direction, making in all twenty stations; and in the Conway bridge, stations in each line at the piers and in the middle of the tube, and also distant stations in the prolongation of the axis of the bridge, making eight stations in all.

The means of the results at each station are given in a Table. For further treatment, the means of those means for collateral stations are used; and the means of those for the two distant stations at each bridge are adopted as giving undisturbed local constants. By treatment of

these in steps exhibited in several successive Tables, results are found for the magnetic disturbance in the three directions, longitudinal, transversal, and vertical; and these are compared with the terrestrial forces in the same directions. The principal results are, that the transversal and vertical parts of terrestrial magnetism are absolutely neutralized, and the terrestrial longitudinal force is diminished by about one fourth part.

An anomaly presented itself in the results for the station at the middle of the Anglesey Wate-tube, which induced the author to inquire of Edwin Clark, Esq. (the actual Superintendent of the construction of the bridges), whether any peculiarity of material could explain it. Mr. Clark immediately remarked that this was the tube which, in the act of raising the sections of the tube, sustained a fall of several inches, undoubtedly straining the tube very greatly. It appears almost beyond doubt that this accident is the cause of the anomalous magnetic condition of that section of the tube. Mr. Clark remarks that it is an interesting fact, if established, that the result of an accident which occurred about a quarter of a century ago can still be traced in the magnetic condition of the tube.

The results in the Conway bridge, for transversal and vertical action, are in perfect agreement with those for the Britannia bridge; but the results for longitudinal disturbance do not present the same agreement. The Conway bridge, however, is nearly at right angles to the magnetic meridian; and it appears probable that its magnetical state depends on the circumstances of its construction, when its Holyhead end was in a direction about $48^{\circ} 50'$ west of north, in which position it would receive much magnetism, which the forces subsequently acting on it could not remove.

II. "On the Organization of the Fossil Plants of the Coal-measures.—Part IV. *Dictyoxyylon*, *Lyginodendron*, and *Heterangium*." By W. C. WILLIAMSON, F.R.S., Professor of Natural History in Owens College, Manchester. Received December 4, 1872.

(Abstract.)

In 1866 Mr. Binney gave the name of *Dadoxyylon Oldhamium* to a fossil stem of a plant from the Lower Coal-measures of Lancashire, believing it to belong to the same class of Gymnospermous Exogens as the *Pinites* of Witham and the *Dadoxyylon* of Endlicher. In 1869 the author pointed out that the reticulated markings upon the surface of its vessels were modifications of the spiral fibre of fibro-vascular tissue, and not the disks of what is often designated glandular fibre. He consequently separated the plant from the *Dadoxyylons* under the name of *Dictyoxyylon Oldhamium*. At the Edinburgh Meeting of the British Association in 1871 he gave a brief account of the structure of this plant, as also of what appeared to be a second species from the Lower Coal-measures of Burntisland in Fife—

shire, which he called *D. Grievii*, after its discoverer, D. Grieve, Esq. A detailed exposition of the organization of these two plants is given in the memoir.

Dictyoxyylon Oldhamium.—This was a stem composed of the three divisions of pith, wood, and bark. The pith consisted of regular parenchyma without divisions or cavities of any kind. In very young plants it was surrounded by an irregular ring or medullary cylinder of reticulated vessels, not arranged in radiating laminae. This cylinder broke up at an early period into several detached vascular bundles, which, as the stem enlarged, became widely separated from each other, the intervening space being occupied by medullary parenchyma. But before this change was completed, the true ligneous zone appeared as a thin ring of vessels arranged in radiating vertical laminae, separated from each other by large and conspicuous medullary rays, composed of mural cellular tissue. Additions were made to the exterior surface of this zone by the agency of a delicate cellular layer of cells, which constituted the innermost layer of the bark. These additions demonstrate their exogenous nature in several specimens in which the vessels of the outermost zone have not attained to half their normal size, resembling in this respect some of the *Lepidodendroid* plants described in the author's last memoir (Part III.). Through these successive exogenous growths the vascular axis of the stem ultimately became arborescent. One specimen is described in which such a vascular axis, though imperfect and waterworn, is fully six inches in diameter, independent of the bark; other specimens have been obtained intermediate in size between the above example and the small stems more usually met with. The vascular laminae increased in thickness as they proceeded from within outwards, and then subdivided, in the ordinary exogenous manner, through the intercalation of new medullary rays. These rays are remarkable for the great vertical range of each one, as well as for the large number of cells which enter into their composition. In tangential sections they appear as elongated lenticular masses of parenchyma.

The Bark.—This organ is separable into three, if not four layers. The innermost one is a delicate parenchyma closely investing the ligneous zone, its cells being continuous with those of the medullary rays. At its outer surface this tissue passes gradually into another parenchymatous layer of greater thickness than the inner one. Both of them have patches of dark-coloured cells scattered through their tissues. But the most remarkable part of the bark is the third or prosenchymatous layer, which presents very different features according to the aspect in which it is regarded. In the transverse section it consists of radiating bands of parenchyma alternating with narrower and very dark-coloured ones of woody prosenchyma, the latter looking very like the Roman numerals upon the face of a clock. Tangential sections show that the black bands are fibrous laminae, which pursue an undulatory course as they ascend

through the stem, and which, as they alternately approach and recede from one another, divide this part of the bark into a series of lenticular or rhomboidal areas, occupied by various forms of parenchyma. No vascular bundles enter these areolæ; hence they are something altogether different from the leaf-scars of the *Lepidodendra*. Externally to this prosenchymatous layer some specimens exhibit detached traces of a very thin external layer of parenchyma, apparently derived from the cells of the rhomboidal areolæ, which have extended beyond the fibrous laminæ and spread themselves over the surface of the bark as a continuous layer; but this condition appears to be confined to very young stems.

Vascular bundles of large size ascend vertically through the two inner parenchymatous layers of the bark. In some instances each of these bundles exhibits, in the transverse section, an oval outline, with faint traces of a vertical division into two parts. But ordinarily the two halves of the bundle have separated, forming two distinct bundles, which are some distance apart. They exhibit little or no tendency to diverge from the ligneous cylinder as they ascend, and in some instances actually become incorporated with it. It is remarkable that the position of each of these double bundles, at the *exterior* of the ligneous zone, corresponds with the spaces intervening between the detached masses of the medullary cylinder *within* it, as if the former were designed to act as buttresses strengthening these weaker points in the vascular axis. It not unfrequently happens that exogenous additions are made to such of these bundles as are encompassed by the innermost layer of the bark, in the shape of a few radiating laminæ of vessels developed on their outer or peripheral surfaces.

One specimen of the vascular axis is, as already mentioned, so large as to demonstrate that the plant became arborescent.

Though *Dictyoxylon* was not a dichotomizing plant, like *Lepidodendron*, it gave off lateral bundles of vessels. Some of these are simple bundles, consisting of numerous vessels intermingled with some cellular tissue. Others have this central bundle invested by a thin ring of radiating laminæ with intervening medullary rays; this exogenous ring sometimes becomes developed into a relatively large and distinct woody zone, like that of the parent axis. The vessels of these lateral growths appear to be wholly derived from the radiating woody zone.

A second form of lateral appendage appears to spring from the medullary rays, and consists of a cylindrical mass of reticulated cells, which are chiefly prosenchymatous, but of an elongated type. It is suggested that this structure may have been prolonged into an adventitious root.

The structure of the central or medullary vascular axis of the former of these two kinds of lateral appendages seems to indicate that the history of the development of the medullary vascular cylinder in these plants corresponds with what the author described in his preceding memoir (Part III.) as taking place in the similar parts of the *Lepidodendra*, viz. that

some of the cells of the central part of the axis underwent rapid fission, and thus developed a distinct cellular medulla, which forced the medullary vessels outwards where at first they constituted a ring, but which ring soon broke up into the detached vascular masses already referred to as adhering to the inner surface of the exogenous zones.

The enlargement of the exogenous woody cylinder by the peripheral intercalation of new radiating vascular laminae, and the repeated subdivision of these laminae by a corresponding intercalation of new medullary rays, demonstrates the close resemblance between the growth of the ligneous zone in these plants and that of ordinary exogenous stems. A fine series of specimens collected by the Rev. H. Higgins, of Rainhill, near Liverpool, and which exhibit various modifications of the type figured by the late Mr. Gourlie under the name of *Lyginodendron Landsburghii*, are shown to be merely casts of the exterior surface of the bark of some species of *Dictyoxyylon*. They may actually belong to *D. Oldhamium*; but this is not yet proven.

Dictyoxyylon Grievii.—This plant has many points of affinity with *D. Oldhamium*; nevertheless it has very distinct features of its own. Its central or medullary axis is very large in proportion to the thickness of its exogenous ring; the former consists of cellular parenchyma, throughout which are scattered numerous bundles of exquisitely reticulated vessels unprovided with any special sheaths. The largest vessels are nearest the centre of the axis, the peripheral ones becoming smaller, more numerous, and grouped in more continuous masses. Immediately surrounding this vasculo-cellular axis is a thin ring of similar vessels, but arranged in radiating laminae, separated by well-defined medullary rays. This zone is generally of unequal thickness on opposite sides of the plant, and contains some barred vessels amongst its reticulated ones; the medullary rays are composed of mural cells.

The bark consists of three very distinct layers. The innermost one is very thin, consisting of delicate parenchyma, but which nevertheless has formed a very clearly defined flexible layer; outside this is a thick stratum of coarser but regular parenchyma subdivided in the transverse section into vaguely defined areas by thick wavy lines of condensed cells. The peripheral outline of this zone is very irregular, frequently projecting outwards in large angular masses. It is bounded by a prosenchymatous external layer, which is a dwarfed representative of the corresponding one of *D. Oldhamium*. In the transverse section it exhibits dark radiating bands of fibres, longitudinally disposed, alternating with similar bands of parenchyma; but it differs from *D. Oldhamium* in the narrowness of the latter, and consequently in the more linear form of the cellular areolae of the outer bark. In longitudinal sections of the bark its innermost layer appears as in transverse ones. The middle parenchyma, on the other hand, exhibits remarkable differences from its aspect in the transverse section: its cells are arranged in vertical columns; but these are inter-

sected at intervals of nearly $\frac{1}{10}$ of an inch by horizontal and parallel bands of very dark-coloured cells of a special nature.

Seven or eight large vasculo-cellular bundles exist in each transverse section of the bark. Some of these are located within the exogenous layer of the wood, being obviously detached portions of the cells and vessels of the medullary axis; others occur, in various specimens, at every point between the wood and the outer bark. The author finds that these bundles remained for a time in the immediate neighbourhood of the innermost bark, but that they successively became detached and moved more rapidly outwards, until each one emerged at the periphery of the bark in one of the prominent angles of the latter, already referred to; when one bundle has thus reached the periphery, another begins to follow the same centrifugal course. The inference is, that these are foliar bundles, supplying large leaves or petioles, sparsely grouped round the stem. A single example of a similar centrifugal bundle was found in *D. Oldhamium*. The seemingly irregular projections of the bark of *D. Grievii* thus appear to represent angular petioles, and are not the result of merely accidental pressures. A second kind of cylindrical bundle is noticed, consisting of reticulated prosenchymatous cells. It is connected at its central extremity with the medullary parenchyma, whilst its peripheral end passes outwards through the bark. It appears to have had the same character as the similar one of *D. Oldhamium*, having probably been an adventitious root-bundle.

Somewhat triangular twigs or petioles of the above plant are numerous. They consist of a single vascular bundle, located excentrically near the cordate base of the triangular transverse section, and surrounded by the three bark-layers seen in the older stems. The structure of these layers, as seen in the longitudinal sections, is identical with, though less complex than, that of the matured stems; but no cortical vascular bundles are seen in them.

Having identified his *Dictyoxydon Oldhamium* with the older genus *Lyginodendron*, the author abandons his own generic name, and proposes that the plant shall henceforth be designated *Lyginodendron Oldhamium*. He establishes in the same way the generic identity of *Dictyoxydon Grievii* with the *Heterangium* of Corda; hence that plant must now take the name of *Heterangium Grievii*. Whilst having no doubt that the above were two Cryptogamic plants, it appears impossible for the present to determine to what class of Cryptogams they belong. Many of their features indicate Lycopodiaceous affinities; but this point can scarcely be determined until the actual fronds are discovered. This has not yet been done. The *Lyginodendron* is from the horizon of the Ganister beds of Lancashire and Yorkshire; the Burntisland deposit belongs to the middle portion of the calciferous sandstones of the Burdiehouse Carboniferous strata.

III. "Observations on the Temperature of the Arctic Sea in the neighbourhood of Spitzbergen." By JOHN C. WELLS, Capt. R.N. Communicated by Dr. CARPENTER, F.R.S. Received November 28, 1872.

While almost every other nation is making its effort to promote scientific discoveries in the high latitudes of the north, England remains inactive.

My object in addressing the following remarks to the Royal Society is to bring to its notice the voyages of the 'Samson' yacht, and to direct attention to some points on the temperature of the Arctic Sea, to which access is obtained through the broadest gateway to the north, *i. e.* that between Greenland and Norway, the portal of which is guarded by Spitzbergen. In the western portion, along the coast of Greenland, it is more or less blocked with ice, and the water is cold. In the eastern part, in the vicinity of Spitzbergen, there is warm water and an open sea at certain seasons of the year as far north as 81° , and in some years one or two degrees further. Nearly all the discoveries in these regions have been made by persons engaged in commercial enterprise; so that, even when favourable opportunities offered, their interests restrained them from taking advantage of the same.

In 1871 Mr. B. Leigh Smith made a cruise in his schooner yacht 'Samson,' and reached $81^{\circ} 24' N.$, with an open sea before him, comparatively free from ice. The pack-ice was drifting southwards, and the water at the surface was $33^{\circ} F.$, while at 300 fathoms it was $42^{\circ} F.$ This fact was observed by Capt. Scoresby in lat. $78^{\circ} N.$, $0^{\circ} 10' W.$, surface $32^{\circ} F.$, and at a depth of 760 fathoms $38^{\circ} F.$ In 1872 a second cruise was undertaken, in which I had the pleasure of accompanying Mr. Smith. On this occasion the sea was crowded with ice, and the ship was beset. The ice had evidently required more than one year for its formation; its surface was covered with opaque snow, and was generally flat, and in no case rose higher than the gangway of the little schooner.

Owing to the floes presenting a comparatively smooth surface, with a total absence of icebergs, we were led to form the opinion that no land can exist in the vicinity immediately north of Spitzbergen, as the southerly drift would be sure to bring down floating bergs, which are always formed in the valleys of northern land.

On this occasion observation with the Miller-Casella thermometer confirmed the result of the previous year, *viz.* gradual increase of temperature at great depth. On July 12th, when in $80^{\circ} 17' N.$, and when the vessel was fixed in the ice, the temperature gradually increased to $64^{\circ} F.$ at a depth of 600 fathoms. These facts indicate the southward flow of a vast body of warm water. It cannot be said that the heat is derived from the Gulf-stream, because nowhere in its course, even in such lati-

tudes as 50° or 60° , does it acquire so high a temperature, even at the surface; and it is highly improbable that the general warmth of the ocean along the west coasts of North Europe, on the shores of Norway, could possibly be supplied by the limited body of warm water which leaves the Gulf of Florida. If the whole of the Gulf-stream water were spread over the warm-water area in the north, its depth, even allowing the most liberal estimate for its volume, would not exceed 10 fathoms; whereas warm water of 42° F. occurs to the depth of 400 fathoms in this region, and north of Spitzbergen it is found as high as 64° F. at 600 fathoms. If it be said that this temperature is due to the northward drifting of the Atlantic from warmer localities, we are met by two difficulties, of which one is, that the soundings obtained by Carpenter and others gave temperatures much below 64° , and the other is, that the waters flow south, not north. Volcanic action, or a warm mineral spring rising from the ocean-bottom, may by some be imagined to be the cause of the temperature of 64° ; but there is no evidence of either of these agencies, and it is quite reasonable to suppose any other feasible cause. Passing over the discovery of 64° F. at this depth, we still have to account for the water of 42° F. flowing southwards, as evidenced by the increase of its temperature as we proceeded northwards.

It is clear that this question of temperature requires further investigation; and it is also clear that whatever the result may be, it will materially affect all the prevailing theories respecting oceanic currents. It is not improbable that this warm water flows from the circumpolar region; and if so, it would indicate a circumpolar sea.

Many facts are known which are consistent with this view. Every year the edge of the pack-ice, and the ice-fields themselves, break up and drift south, at a rate sometimes equal to thirteen miles a day, as found by Capt. Parry. This does not occur when the northern ocean is wholly covered with ice, in the winter season. The drifting of the ice (as also currents) implies a sea free of ice somewhere in the north, occupying an area at least as extensive as the drift-ice. As has been seen, some of the ice is the result of more than one year's growth; and as the ice travels southerly, say, from four to thirteen miles or more per diem, a similar area of open sea must be simultaneously forming round the pole, the ice-holes and clear spaces in the drift-ice being quite insufficient to make up for the space left by the ice during the summer. The great abundance of animal life in the waters of the highest latitudes reached indicates that the water is not ice-cold; and the migration of numerous species to the north of 80° shows that the means of subsistence can be obtained. There is reason to believe that whales occur far to the north of 80° ; and if so, there must necessarily be sufficient open water to allow of their finding ready access to air.

In the Spitzbergen seas a blue, cloud-like appearance is well known as a sign of open water; and this has been seen on the distant north horizon

even by ships which have been beset by ice in the highest latitudes. Icebergs, it is well known, waste more rapidly below the surface than in the air, causing them to topple over frequently, obviously the effect of the warm current.

The question, then, is, from whence do the warm waters come? and how do they acquire their heat? And this is one of the questions which a polar voyage by way of Spitzbergen would almost certainly elucidate. Another important subject of investigation would be the conditions under which the prevalent north winds of high latitudes originate. There is one argument bearing upon the temperature of the circumpolar seas which should not be overlooked. During six months of the year the sun is above the horizon; and although the rays may be oblique, still the waters may acquire a higher temperature than under similar conditions further south, owing to there being little or no cooling from nocturnal radiation, and probably to the constant dryness of the air allowing the sun to strike with full power. During the winter these causes would intensify the cold.

The occurrence of warm water is by no means confined to the sea around Spitzbergen; but, before referring to other regions, we may mention that a set of instruments for taking soundings and deep-sea temperatures was supplied this year by Mr. Smith to Capt. David Grey, of the whaler 'Eclipse,' whose father, in the year 1855, supplied the valuable information and survey of the extension of Pond's Bay, now called Eclipse Sound. His observations were made in the middle of the sea, between Greenland and Norway, and along a line running north-easterly from Iceland. They coincide with Dr. Carpenter's observations, proving the termination of the Gulf-stream. In June 1854 Morton advanced beyond Kennedy Channel, and saw open water as far as the horizon, visible from a hill 500 feet high. The wind was from the north-west, and a rain-cloud was seen in the distance above the open sea. The water was setting in a strong current south, and the ice along the shores was in a rapid state of dissolution. The water was found in the several places tried to be well above the freezing-point; and in one place, some distance from the ice-foot, and at a depth of 5 feet, the temperature was 40° F. There was a strong tide from the north. Kane's vessel wintered in Rensselaer Harbour; the strait was bridged across by ice, with a current running south flowing beneath it. Although the open waters above alluded to may not be direct evidence of a comparatively mild circumpolar region, yet the stream of warm water coming from the north seems to indicate it.

Where can this water acquire its warmth? Sir John Richardson suggests that it is derived from the warm area near Spitzbergen; but this is not supported by evidence, which indicates that in both areas the water comes from the north. It has been suggested that it is a continuation of the Gulf-stream, apparently because it is supposed to supply all the warm water in the Arctic seas; but if there is no reason for believing that the warm sea around Spitzbergen derives its heat from this source, it is still

less credible in the case of the Kennedy-Strait water. It has been suggested that the source of warmth is the northward flow of the general mass of the North Atlantic. If this did account for the warmth of the Spitzbergen area, although this view would be with difficulty reconciled with a southward flow of Arctic water, it would be quite inapplicable to the Kennedy-Channel area.

Temperatures taken by Mr. B. Leigh Smith and Capt. Wells, R.N.

1872.	Station.	Lat.	Long.	Depths in fathoms.	Temperature.			
					Air.	Sur- face.	Min.	Max.
June								
1.	1.	68 52 N.	6 40 W.	600	42	37½	30	37½
13.	2.	75 6 N.	2 30 W.	100	36	31	28	35
15.	3.	75 7 N.	3 48 W.	100	36	32	28	35
				50	...	31	29½	32½
17.	4.	76 13 N.	2 22 W.	100	34	31	29½	34
18.	5.	76 3 N.	0 10 E.	150	35	33	30½	40
				200	...	33	30½	48
19.	6.	76 21 N.	1 5 E.	150	35	32	30½	32
				250	...	32	30½	39½
20.	7.	76 35 N.	0 3 W.	6	34	33	30	33
				25	...	33	30	35
				150	...	33	30	39½
22.	8.	76 41 N.	2 10 W.	150	35	32	29½	39½
27.	9.	77 18 N.	5 0 E.	25	37	34½	32	34½
				250	...	34½	32	39
July								
1.	10.	78 20 N.	7 2 E.	6	36	36	33	36
			"	600	...	36	33½	36½
6.	11.	79 54 N.	6 34 E.	6	35	34½	33	34½
				12	...	34½	33	35
				25	...	34½	33	37
				50	...	34½	33	37
				200	...	34½	33	40
				Bottom				
7.	12.	80 4 N.	5 12 E.	600	37	34½	31½	39
10.	13.	80 23 N.	9 0 E.	12	35	31	28	31
				50	...	31	28	31½
				Bottom				
12.	14.	80 32 N.	9 50 E.	600	36	31	28½	64

Among the many advantages that would result from circumpolar research and the following up of this warm current, not the least important would be the insight which it would probably afford as to the regulating influences of the weather of North Europe, or generally of the northern hemisphere. Meteorologists have long suspected that the weather in Western Europe depends in some way upon what has happened in the vicinity of the pole. The many advantages to be gained to science by circumpolar navigation cannot be doubted. Among them would be careful observations of the currents and temperatures of the surface and at various depths, and organisms which doubtless would be obtained by dredging, as far as practicable, in the bed of the Arctic Sea, in the highest latitude, and the probable extension of the whale-fisheries, as well as the discovery of new land, should such exist.

June 1.—The edge of the ice was 170 miles distant, and the warm water was found at the surface, and cold water, which is of greater density, below.

June 13.—The sounding was taken at the edge of the pack. If the experiments had been continued, increasing temperature would probably have been found at a lower depth, as was the case further north.

June 15.—To-day we were well in the ice, and had only time to sound in 50 fathoms; but even here we found an increasing temperature.

June 17.—Being far in the ice, we only found a slight increase.

June 18.—A second sounding, 50 fathoms deeper than yesterday, when sailing among large pieces of floe-ice, gave an increasing temperature up to 48° F.

June 19.—To-day we had an increase of 7° above the surface-temperature at 250 fathoms deep, although but 10 miles distant from yesterday's soundings; but the ice was more open: probably evaporation occurred here, owing to the surface not being so closely covered with ice.

June 20.—The first sounding shows the water to be coldest at the ice-foot; the second and third prove the increasing temperature, which, as we were somewhat more closely packed in the ice, increased more quickly, and 39° F. is obtained at 150 fathoms deep.

June 22.—Here we were more closely packed. The surface was a degree colder than yesterday, and the water at the ice-foot had also gone down half a degree; but the warm stream below was the same. If we had had time to sound at greater depths, the temperature of the water would in all probability have been found still on the increase.

June 27.—In this case we have the warm current clearly defined at 250 fathoms depth, being kept below by the lighter ice-water, which is nearly fresh and of less specific gravity.

July 1.—We were clear of the ice, and had a lower temperature than before obtained. If the flow of warm water came from the south, the surface-temperature would have been greater than any temperature as yet obtained, because of the absence of ice.

July 6.—This observation shows a gradual increase as far down as 200 fathoms. We were well in the ice, and were prevented from continuing our sounding-operations. Little evaporation has taken place, owing to the closeness of the ice and the gradual increase of the temperature, with one exception, which occurred on the 18th of June, when a temperature of 48° was obtained,—showing that the temperatures are materially affected by local causes, the closeness of the pack or the opening of the ice allowing evaporation to take place.

July 7.—Here, where the ice is close, there is a great difference between the surface temperature and that at the ice-foot. We pulled up a very beautiful description of starfish, which would not be likely to inhabit this warm current were it of volcanic origin.

July 10.—The crew being required elsewhere, we could not continue our sounding-experiments. The sounding shows a decrease of temperature, owing to the ice-water.

July 12.—This remarkable sounding was carefully registered specially by Mr. Smith, who saw the index before it was immersed and immediately on its coming up. This shows a gradual increase of temperature towards the north, proving the current to come from the north; and its temperature being above that of the Gulf-stream, where it disperses itself, is a proof it is in no way connected with it.

The thermometer has since been examined by Mr. L. P. Casella, who certifies that it “has been tested in the hydraulic press, as well as carefully compared with my standard, and found correct in every way, no change whatever having taken place in the instrument.”

Temperatures taken by Capt. David Grey, of the 'Eclipse' Whaler.

1872.	Station.	Lat.	Long.	Depths in fathoms.	Temperature.			
					Air.	Sur- face.	Min.	Max.
April 13.	15.	68 45 N.	13 58 W.	220		29	28.8	32
15.	16.	68 52 N.	15 40 W.	220		29	29	31.5
20.	17.	68 12 N.	16 40 W.	270		28.8	28	31
May				Bottom				
14.	18.	75 0 N.	16 20 E.	85		29	29	32
June								
6.	19.	78 20 N.	0 20 E.	400		30	30	33
8.	20.	78 14 N.	0 18 W.	220		30	29	30
				400		30	30	32
18.	21.	75 5 N.	6 15 W.	400		32	29.5	32
				200		32	30	32
23.	22.	74 50 N.	6 50 W.	100		32	30	32
				400		31	30	32
July 3.	23.			200		34	30.5	34

April 13, 15, 20.—When these three soundings were taken the ship was frozen in the pack; still there is a slight increase of temperature at the lowest depth.

May 14.—Off Bear Island.

June 6, 8.—Here 'Eclipse' was 75 miles inside the pack.

June 18, 23, July 3.—Made fast to a large floe, about 90 miles inside the pack.

The Society then adjourned over the Christmas Recess to Thursday, January 9, 1873.

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January 9, 1873.

WILLIAM SHARPEY, M.D., Vice-President, in the Chair.

The following communications were read :—

- I. "On a new Method of viewing the Chromosphere." By J. N. LOCKYER, F.R.S., and G. M. SEABROKE. Received November 6, 1872.

The observations made by slitless spectroscopes during the eclipse of Dec. 11, 1871, led one of us early this year to the conclusion that the most convenient and labour-saving contrivance for the daily observation of the chromosphere would be to photograph daily the image of a ring-slit, which should be coincident with an image of the chromosphere itself.

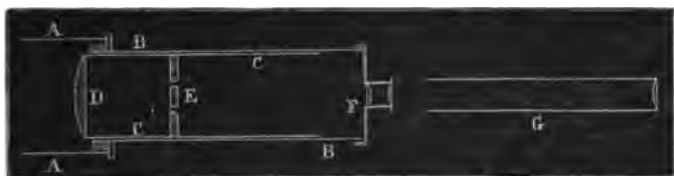
The same idea has since occurred to the other.

We therefore beg leave to send in a joint communication to the Royal Society on the subject, showing the manner in which this kind of observation can be carried out, remarking that, although the method still requires some instrumental details, which will make its working more perfect, images of the chromosphere, almost in its entirety, have already been seen on several days during the present month and the latter part of last month.

The adaptation of this method to a telespectroscope will be seen at a glance from the accompanying drawing.



Diaphragm showing annulus, the breadth of which may be varied to suit the state of the air.



The annulus is viewed and brought to focus by looking through apertures in the side of the tubes.

- A. Sliding eye-tube of telescope. B. Tube screwing into eye-tube. C. Tube sliding inside B, and carrying lens D and diaphragm E. F. Lenses bringing image of diaphragm to a focus at the place generally occupied by the slit of the spectroscope. G. Collimator of spectroscope.

The image of the sun is brought to focus on a diaphragm having a circular disk of brass (in the centre) of the same size as the sun's image, so that the sun's light is obstructed and the chromospheric light is allowed to pass.

The chromosphere is afterwards brought to a focus again at the position usually occupied by the slit of the spectroscope; and in the eyepiece is seen the chromosphere in circles corresponding to the "C" or other lines. The lens D is used to reduce the size of the sun's image, and keep it of the same size as the diaphragm at different times of the year; and the lenses F are used in order to reduce the size of the annulus of light to about $\frac{1}{8}$ inch, so that the pencils of light from either side of the annulus may not be too divergent to pass through the prisms at the same time, and that the image of the whole annulus may be seen at once. There are mechanical difficulties in producing a perfect annulus of the required size, so one $\frac{1}{8}$ inch in diameter is used, and can be reduced virtually to any size at pleasure.

The proposed photographic arrangements are as follows :—

A large Steinheil spectroscope is used, its usual slit being replaced by the ring one.

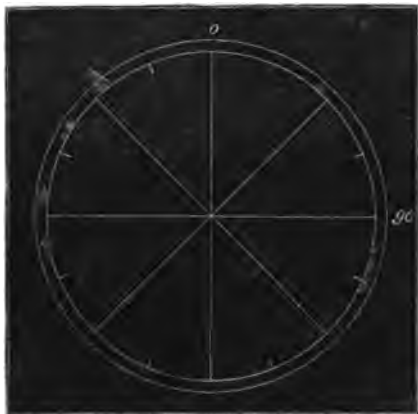
A solar beam is thrown along the axis of the collimator by a heliostat, and the sun's image is brought to focus on the ring-slit by a $3\frac{3}{4}$ -inch object-glass, the solar image being made to fit the slit by a suitable lens.

By this method the image of the chromosphere received on the photographic plate can be obtained of a convenient size, as a telescope of any dimensions may be used for focusing the parallel beam which passes through the prisms on to the plate.

The size of the image of the chromosphere obtained by the method adopted will be seen from the accompanying photograph, taken when the ring-slit was illuminated with the vapours of copper and cadmium.

As this photograph is not reproduced, it may be stated that the ring-images have an internal diameter of nearly $\frac{3}{4}$ of an inch.

December 6, 1872, at 11.30.



December 7, 1872, at 11.30.



Outer circle 100" from inner one. Chromosphere at normal height, except where prominences marked.

The accompanying solar profiles are copies of drawings made, on the dates stated, by means of the new method, which were exhibited by the authors at the Meeting.

[Since reading the above paper, it has come to our knowledge that Zöllner had conceived the same idea unknown to us, but had rejected it. Prof. Wenlock in America has tried a similar arrangement, but without success.—J.N.L., G. M.S., January 17, 1873.]

II. "Further Remarks on the Sense of Sight in Birds." By ROBERT JAMES LEE, M.A., M.D. Communicated by ROBERT LEE, M.D., F.R.S. Received October 14, 1872.

It was the object of my last communication to show the necessity for more extensive researches into the differences of certain structures in the eyes of birds in order to arrive at satisfactory conclusions regarding their functions, which are still undecided by anatomists. It also appeared to me to be desirable and interesting to examine the different degrees of development which characterize those structures in different species of birds, as they seemed to afford an explanation of the remarkable powers of vision which most birds possess.

It would be premature to enter upon general deductions until the data we possess are more numerous and the anatomical details are generally allowed to be correct.

Since my last communication it has been my privilege to receive much assistance and valuable information from Mr. Hulke, who has directed considerable attention to the structure of the ciliary muscle in birds.

It appears to me impossible to reconcile the various opinions which Brücke, Cramer, Donders, and particularly Heinrich Müller entertain on anatomical details, except on the supposition that the exact species of bird has not been considered of sufficient importance to the question, also that the particular method of dissection or preparation may have been the cause of a different appearance in the minute structures. In my own experience I have often found that singular differences are produced by the method of preparation; and it has usually been necessary, where there was any doubt, from the eye having been preserved for some time, to wait till a fresh specimen could be obtained.

In order to show the different degrees of development of the ciliary muscle, a short Table has been drawn up, containing those specimens which have been examined with most attention. For the present I shall consider the ciliary muscle as a simple structure for the production of one effect, whatever minute differences may exist in the internal arrangement of its fibres.

The second Table may be regarded as a continuation of that commenced in my last communication, and is intended to furnish certain data which are necessary for the determination of the visual powers in various species of birds.

	Axis of Vision.	Ciliary Muscle.		Axis of Vision.	Ciliary Muscle.
Eagle Owl	3.7	1	Vieillot's Pheasant ..	6	1
Vulture	3.1	1	Wood Francolin	4.6	1
Buzzard	4	1	Canada Goose	5	1
<i>Rhea americana</i> ..	3	1	Hawk-headed Parrot	4	1
Flamingo	9	1	Spotted Dove	7	1
Penguin	6	1	Grouse	4	1
Andean Goose ..	4	1	Partridge	4	1

l. = lateral; v. = vertical; a.p. = antero-posterior; unit of measurement 1 inch; the sign + denotes that actual size is *greater* than the mean of the two numbers, - that it is *less* than the mean.

	Cornea.	Sclerotic.	Lens.	Muscle.	Post. elastic ligament.
Andean Goose ..	l. $\frac{1.5}{4.0}$	l. $\frac{1.5}{2.0}$	l. $\frac{1.0+1.1}{3.2}$	$\frac{1}{8}$	$\frac{1}{16}$
	v. $\frac{7}{20}$	v. $\frac{1.4}{2.0}$	a.p. $\frac{7}{3.2}$		
		a.p. $\frac{1}{2}$			
Vieillot's Pheasant	$\frac{1.3}{3.2}$	l. $\frac{2.7}{3.2}$	l. $\frac{1.0-1.1}{3.2}$	$\frac{3}{3.2}$	$\frac{3}{6.4}$
		v. $\frac{2.6}{3.2}$	a.p. $\frac{7}{3.2}$		
		a.p. $\frac{9}{16}$			
Wood Francolin	l. $\frac{9}{3.2}$	l. $\frac{9}{16}$	l. $\frac{7}{3.2}$	$\frac{3}{3.2}$	$\frac{3}{6.4}$
	v. $\frac{8}{3.2}$	a.p. $\frac{7}{16}$	a.p. $\frac{1.1}{6.4}$		
Canada Goose ..	v. $\frac{9}{20}$	l. $\frac{2.9}{3.2}$	l. $\frac{1.1}{3.2}$	$\frac{1}{8}$	$\frac{1}{8}$
	l. $\frac{8}{20}$	v. $\frac{1.4}{16}$	a.p. $\frac{8}{3.2}$		
		a.p. $\frac{2.0}{3.2}$			
Hawk-headed Parrot	$\frac{9}{3.2}$	$\frac{2.1}{3.2}$	l. $\frac{1}{4}$	$\frac{7}{6.4}$	$\frac{1}{3.2}$
		a.p. $\frac{1.5}{3.2}$	a.p. $\frac{1.3}{6.4}$		
Chatterton's Francolin	$\frac{9}{3.2}$	l. $\frac{1.9}{3.2}$			
		a.p. $\frac{1.5}{3.2}$			
Spotted Dove	$\frac{3}{10}$	l. $\frac{1.2}{2.0}$	l. $\frac{4}{2.0}$	$\frac{1}{16}$	$\frac{1}{20}$
		a.p. $\frac{9}{2.0}$	a.p. $\frac{3}{2.0}$		
Grouse	$\frac{5}{16}$	l. $\frac{1.1}{16}$	l. $\frac{9}{3.2}$	$\frac{2}{16}$	$\frac{1}{3.2}$
		v. $\frac{2.1}{3.2}$	a.p. $\frac{6}{3.2}$		
		a.p. $\frac{1.5}{3.2}$			
Partridge	$\frac{4}{16}$	l. $\frac{9}{16}$	l. $\frac{4}{16}$	$\frac{1}{16}$	$\frac{1}{3.2}$
		a.p. $\frac{1.1}{3.2}$	a.p. $\frac{1.1}{3.2}$		

III. "On the Union of Ammonia Nitrate with Ammonia." By
EDWARD DIVERS, M.D. Communicated by Professor ODLING,
M.B., F.R.S. Received October 29, 1872.

(Abstract.)

Ammonia nitrate deliquesces in ammonia gas at ordinary temperatures and pressures, forming a solution of the salt in liquefied ammonia. To prepare the product, it is only requisite to pass dry ammonia gas into a flask containing the dry nitrate; but the condensation proceeds more rapidly if the flask is surrounded with ice.

The liquid obtained varies in composition according to the temperature and pressure. At a temperature of 23° and the pressure of the atmosphere, it consists of about four parts of nitrate to one of ammonia by weight; but under greater pressure, or at lower temperatures, much more ammonia can be condensed by the nitrate. At 0° and the pressure of the atmosphere, two parts of nitrate can condense one part of ammonia. Like an aqueous solution, the liquid boils when heated, and, when nearly saturated with the nitrate, deposits crystals of it when cooled. It can also, like an aqueous solution, be heated above its boiling-point without boiling, and become supersaturated with the salt without crystallizing. When poured out into an open vessel, it becomes almost instantly gelatinous in appearance—may, indeed, become so as it falls in a stream from the flask containing it. This effect is due to evaporation of ammonia and solidification of nitrate at the surface of the liquid: on breaking the crust of nitrate, the compound flows out as liquid as ever. It is not caustic to the dry skin. During its decomposition cold is manifested, and during its formation heat is evolved, but not to a great extent, because the heat given out by the liquefaction of the ammonia is nearly all used up in the liquefaction of the nitrate.

The specific gravity of the liquid varies, of course, with its composition. When it consists of two of nitrate to one of ammonia, it has a specific gravity of 1072.5; when it consists of four of nitrate to one of ammonia, it has a specific gravity of nearly 1200. Its specific gravity can be calculated from its composition, by taking for the purpose 1524.5 as the specific gravity of the nitrate, and 671 as that of the ammonia. The number 1524.5 is much less than that expressing the actual density of the nitrate in the solid state, but it does not differ very much from its apparent specific gravity in aqueous solution.

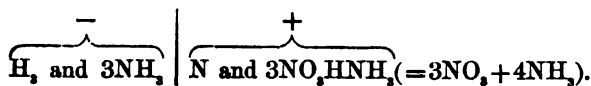
In its rate of expansion by heat, the liquid resembles others that exist as such at ordinary temperatures, rather than those that, like ammonia itself, are only retained as such by great pressure. Its expansivity increases with the quantity of ammonia present.

The volume of a mixture of the liquid with water is much less than the sum of the volumes of the liquid and the water, and yet a marked

absorption of heat occurs during the admixture. The same thing happens when a concentrated aqueous solution of the nitrate is poured into water, as was first pointed out by Gay-Lussac. Other examples of this remarkable phenomenon have been observed by different chemists, and have received various explanations. F. Mohr considers that heat is used up in the depression of the freezing-point of the water which the salt produces. As this depression of the freezing-point is probably attended by an increase in the latent heat of the water, his explanation appears to be the correct one. Thomsen finds the specific heat of the mixture to be less than the mean of the specific heats of its components.

Its action upon a great number of substances, principally inorganic, has been tried, and found to be for the most part like the actions of dry ammonia and ammonia nitrate conjoined. The nitrate appears to undergo double decomposition with most salts, and the ammonia to unite with nearly all, including the salt of magnesium, aluminium, iron, and manganese. The ammoniated chromium salts possess considerable stability. The ammoniated mercuric iodide is resolved by washing into ammonia and mercuric iodide again. The ammoniated compounds which do not dissolve in the liquid are very bulky, as observed by Gore in his experiments upon ammonia liquefied by pressure. Nitrates, chlorides, iodides, and bromides are either soluble, as Gore has found them to be, in ammonia alone, or else are decomposed into soluble chlorides &c. of ammonium, and insoluble ammoniated compounds of the metals. Sulphates, oxalates, chromates, and arsenites are insoluble, and phosphates are nearly so. Phosphoric and chromic anhydrides do not act upon the liquid with the energy that might be expected, but combine with the ammonia. Iodine dissolves freely, as it does in ammonia alone (Gore). Bromine generates nitrogen. Lead salts, including sulphate, chloride, iodide, and oxide, are freely soluble as ammoniated compounds. Platinous chloride dissolves freely as tetra-ammonio-platinous chloride. Potassium salts are very sparingly soluble. Alkalis and their carbonates decompose the nitrate; so do litharge, lime, and baryta. Calomel is converted into metallic mercury and a soluble ammoniated mercuric compound. Potassium, sodium, zinc, and cadmium dissolve without liberating gas, by reducing the nitrate to nitrite, potassium inflaming. Magnesium slowly dissolves, liberating a little hydrogen, reducing the nitrate, and becoming partly converted into Beetz's black suboxide of magnesium. Methyl iodide is decomposed; butyric ether and chloroform are sparingly soluble without decomposition. Ether is insoluble, but by its contact causes the liquid to break up into its two constituents.

It is a good electrolyte, ammonia and hydrogen appearing at the negative electrode, and nitrogen and ammonia nitrate at the positive electrode. Its decomposition may be thus represented:—



Positive electrodes of silver, lead, copper, zinc, and magnesium are dissolved by the liquid as (ammoniated) nitrates. A positive electrode of mercury is converted into a compound almost insoluble in the liquid. When the electrode is acted upon, the generation of nitrogen does not take place.

January 16, 1873.

T. ARCHER HIRST, Ph.D., Vice-President, in the Chair.

Pursuant to notice given at the last Meeting, Mr. Spottiswoode proposed and Admiral Richards seconded the Right Hon. Hugh Culling Eardley Childers, Chancellor of the Duchy of Lancaster, for election and immediate ballot.

The ballot having been taken, Mr. Childers was declared duly elected.

The following communications were read:—

- I. "A new Formula for a Microscope Object-glass." By F. H. WENHAM. Communicated by W. B. CARPENTER, M.D., F.R.S. Received October 31, 1872.

A pencil of rays exceeding an angle of 40° from a luminous point cannot be secured with less than three superposed lenses of increasing focus and diameter, by the use of which combination rays beyond this angle are transmitted, with successive refractions in their course, towards the posterior conjugate focus: until quite recently, each of these separate lenses has been partly achromatized by its own concave lens of flint glass, the surfaces in contact with the crown glass being of the same radius, united with Canada balsam; the front lens has been made a triple, the middle a double, and the back again a triple achromatic. This combination therefore consists of eight lenses, and the rays in their passage are subject to errors arising from sixteen surfaces of glass.

In the new form there are but ten surfaces, and only *one* concave lens of dense *flint* is employed for correcting *four* convex lenses of *crown* glass: as this might at first sight be considered inconsistent with theory, a brief retrospect of the early improvements of the microscope object-glass will help to define the conditions. The knowledge of its construction has been entirely in the hands of working opticians; and the information published on the subject being scanty, this has probably prevented the scientific analyst from giving that aid which might have been expected.

Previous to the year 1829 a few microscopic object-glasses were made, composed of three superposed achromatic lenses; but this combination

appears to have been used merely with the intention of gaining an increase of power, in ignorance of any principle, and without even a knowledge of the value of angular aperture.

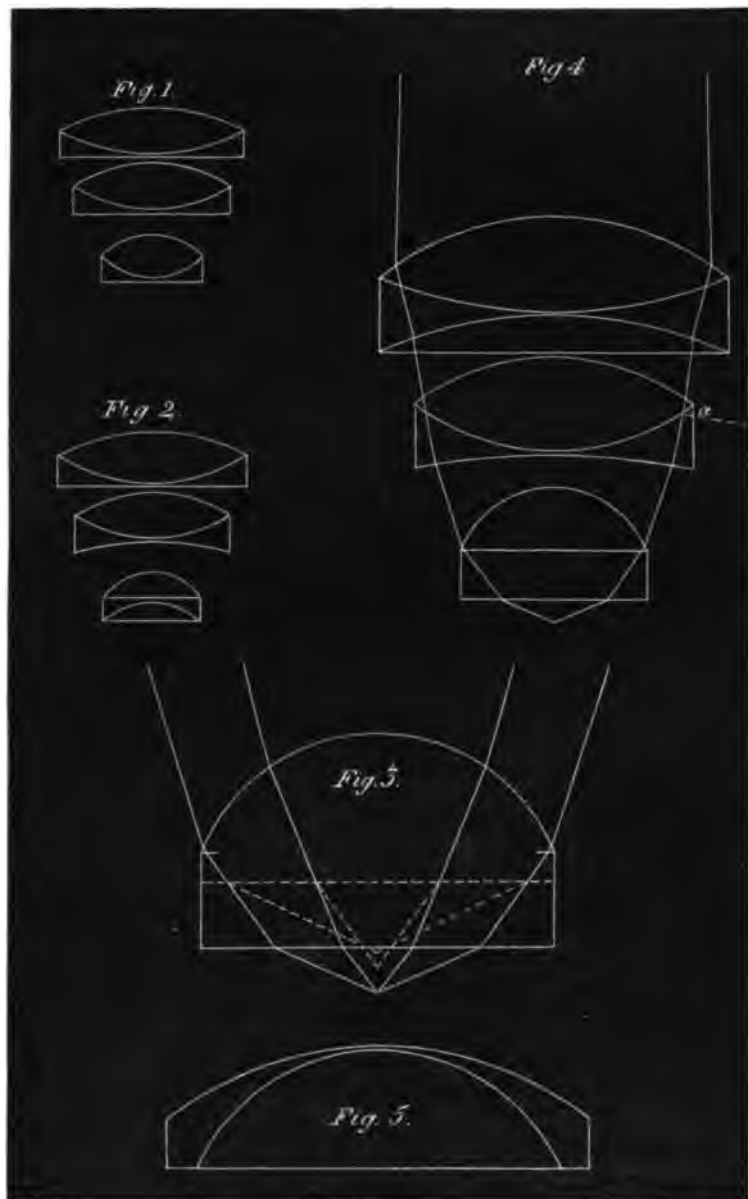
At this time the late J. J. Lister tried a number of experiments, and discovered the law of the aplanatic focus, and proved that, by separating lenses suitably corrected, there were one or two positions in which the spherical aberration was balanced. This was explained in a paper read before the Royal Society in 1829. In the year 1831 Mr. Ross was employed to construct the first achromatic object-glass in accordance with this principle, which performed with "a degree of success never anticipated."

Mr. Ross then discovered that, after he had adjusted the interval of his lenses for the aplanatic focus, that position would no longer be correct if a plate of thin glass was placed above the object; this focus had then to be sought in a different plane, and the lenses brought closer together, in order to neutralize the negative aberration caused by covering-glass of various thickness. From this period the "adjustment" with which all our best object-glasses are now provided became established. Fig. 1 is the form of object-glass used at this time, consisting of three plano-concave achromatics, whose foci were nearly in the proportion of 1, 2, 3.

No greater angle than 60° could be obtained with this system in a $\frac{1}{2}$ -inch objective (the highest power then made), for reasons apparent in the diagram. The excessive depth of curvature of the contact-surfaces of the front pair is unfavourable for the passage of the marginal rays; the softness of the flint glass forming the first plane was also objectionable. In the year 1837 Mr. Lister gave Mr. Ross a diagram for an improved "eighth," having a *triple front lens* in the form shown in fig. 2. By this the passage of extreme rays was facilitated; and in order to diminish the depth of curvature, a very dense glass was used, having a specific gravity of 4.351. Faraday's glass, having a density of 6.4, had been previously tried, but was abandoned on account of a difficulty in working it. The polished surfaces of both these qualities of dense glass speedily became tarnished by exposure to the air; and thus the dense flint concave could only be employed in a triple combination, that is, when cemented between two lenses of crown glass: this form of front was kept a trade secret, and was not published in any work treating of the optics of the microscope. The front incident surface of the flint of the middle pair was made concave, in order to reduce the depth of the contact; and for this reason only, as that surface has but little influence in correcting the oblique pencils, or in producing flatness of field, and may be a plane with an equally good or better result. "Eighths" of this form with angles of 80° were made, and remained unaltered till the year 1850, when larger apertures were called for, and Mr. Lister introduced the *triple back lens*.

The necessity for this will be seen by the diagram (fig. 2), which shows

that the contact-surfaces of the back achromatic are too deep, thus giving great thickness to the lens and limiting its diameter : dense flint



would have remedied this to some extent ; but its liability to tarnish

rendered its use in a *pair* objectionable. The highest density at this time known, quite free from this defect, was 3·686. By means of the triple back, the final corrections were rendered less abrupt, a greater portion of the marginal rays could be collected, and the aperture of an "eighth" was at once brought up to 130° or more.

At this time the author had been making some experiments in the construction of an object-glass in the form of fig. 2. Mr. Lister having favoured his "eighth" with an examination, was good enough to communicate his late improvement of the triple back. No time was lost in giving this a trial, the result of which proved that excessive negative aberration or over-correction could readily be commanded with lenses of shallow contact-curves. During these trials all chromatic correction was obtained by alterations in the triple *back*; for it was found that the colour-correction could not be controlled by a change in the concave surface of the triple *front*, as the negative power of the flint here appeared to be feeble, requiring a great difference in radius to give a trifling result. For this reason the front concaves were formed of very dense and highly dispersive flint; the cause of this was analyzed by a large diagram, with the passage of the rays projected through the combination, starting from the longest conjugate focus at the back. This proved that the rays from that focus passed through the concave flint of the front nearly as a radius from its centre, or in such a direction that its negative influence was almost neutralized. It is well known that a lens may be achromatic for parallel rays, and under-corrected for divergent ones. The utmost extent of this condition was apparent in the object-glass under consideration.

This led the author to the idea of the *single front* lens of crown glass, which gave a fine result at the first attempt, as the back combinations to which it was applied happened to have a suitable excess of negative or over-correction existing in the triple back alone, the middle being neutral or nearly achromatic. Still there was a defect remaining as positive spherical aberration; and this was afterwards cured by giving *additional thickness* to the *front* lens, which is now recognized as a most essential element of correction. In a "fifteenth," for instance, a difference of thickness of only ·002 of an inch will determine the quality between a good and an indifferent glass. Fig. 3 represents a front lens suitable for bringing the back rays to a focus. The dotted lines indicate the effect of this difference, showing that with a lens of less thickness the marginal rays fall within the central, producing positive aberration as the result.

The single front introduced by the author is now used by every maker; for several years he could not induce the leading opticians to change their system, though challenged by a series of high powers constructed on this formula, for the purpose of proving its superiority. Fig. 4 represents the curves of the first successful "eighth" on this system, having an aperture of 130°, enlarged ten times. On tracing the passage of

the marginal rays through the combination, it will be seen that, though the successive refractions are nearly equalized, the contact-surfaces of the middle pair are somewhat deep, though no over-correction existed or was needed here, for this would have required a shorter radius still (the density of the flint in this was 3.686). If this pair of lenses were not cemented with Canada balsam, total reflection would take place near the circumference of the contact flint surface, cutting off the marginal rays at *a*, and limiting the aperture. It might be argued that practically this would be no disadvantage, as these surfaces are united with Canada balsam, whose refraction is higher than the crown; so that the rays in this case must proceed with very little deviation. But incidences beyond the angle of total reflection may be considered detrimental, as they imply excessive depth of curvature; this can be discovered by looking through the front of an object-glass held close to the eye, any air-films in the balsam near the edge of the lens appearing as opaque black spots.

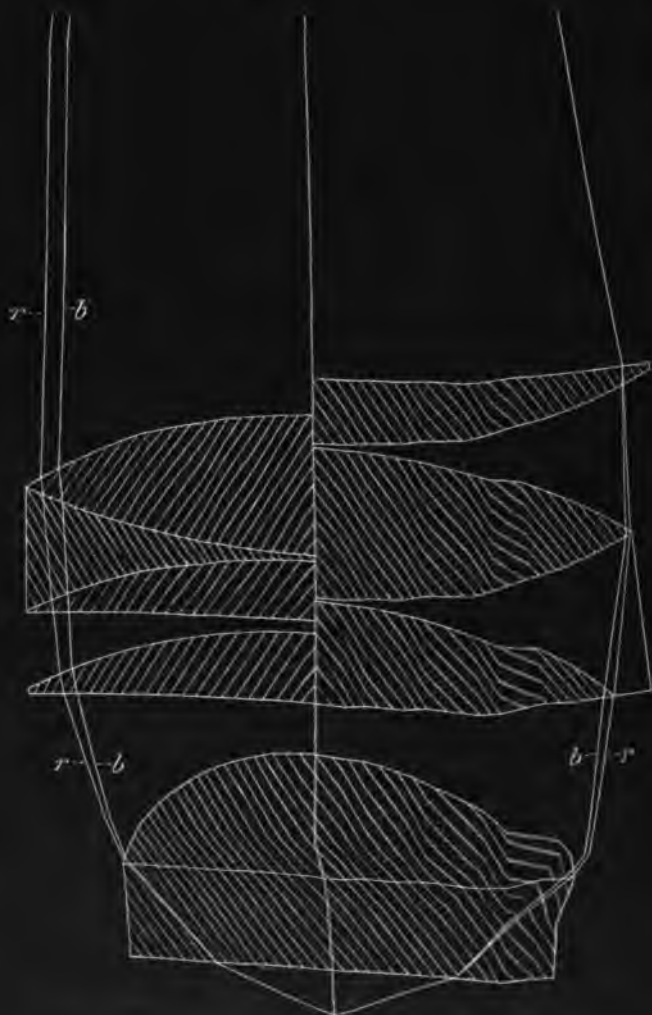
At the commencement of the present year the author caused a few object-glasses to be made, with a middle of the form of fig. 5, the performance of which was very satisfactory. In this the extreme rays pass at more favourable incidences, and within the angle of total reflection. The upper lens is of dense flint.

When the experiments on the single front were concluded, and the remarkable corrective power of the triple back in conjunction therewith had been proved, the next attempt was to make the *middle* also a single lens, leaving the entire colour-correction to be performed by the one bi-concave flint in the back. After numerous trials it was found that though something like over-correction or negative aberration could be obtained with the back, in the degree requisite for balancing the under-correction of the single middle and front when set at the prescribed distance of the aplanatic focus, yet by trial on the mercury globule all the results invariably displayed two separated colour-rings: these could not be combined by any alteration in the radius of the lenses. By projecting the blue and red, or visible rays of greatest and least refrangibility through the system, the cause became apparent. The left-hand section of this object-glass is shown in fig. 6. The rays from the focus are slightly divided by the first front surface. On emerging from the back the separation is increased; the red ray (*r*) is outwards, and the more refrangible or blue ray (*b*) inwards. Next, the divergence of these two rays is extended by the middle single lens. The following crown lens extends the angle of divergence so far that the flint lens of the back triple cannot recombine them; and they emerge at two distinct zones, shown by the practical test of the "artificial star" or light-spot reflected from a mercury globule, viewed within and without the focus.

It might be supposed that these rays at their final emergence can be so refracted as to project the blue outwards. A crossing point would then occur at a fixed conjugate focus in the body of the microscope, at

which all rays would be combined; and if this focus was adjusted to that of the eyepiece, achromatism and final correction would be the

Fig. 6.



result. But to meet the various conditions occurring in the use of the microscope, the conjugate focus constantly alters in position, this

being affected by every change of eyepiece, length of tube, or adjustment for thickness of cover; therefore a correction for a fixed point cannot be maintained. Achromatism in the microscopic object-glass, like that of other perfectly corrected optical combinations, must be the reunion of the rays of the spectrum close to the final emergent surface of the system. The remedy suggested by these experiments appeared to be in a transposition, that is, in placing the over-corrected triple in the *middle* of the entire object-glass; this would at once cause a convergence of the blue and red rays. A single lens of longer focus at the back would then bring these rays parallel at the point of final emergence.

By projection in a diagram this condition was apparently realized. The dispersive power of the flint (density 3.686) was taken by the refractive index 1.76 of line H in the blue ray of the spectrum, and 1.70 of line B in the red ray. The refraction of the corresponding rays in the crown (density 2.44) was 1.53 H and 1.51 B. With these indices the rays are traced in fig. 6. The radii in the right-hand half section are those of an "eighth" of the new form drawn twenty times the size of the original. The *single front* is of the usual form, as this is much alike in all cases. The radius or focus of the *single plano-convex back* is about four and a half times that of the front, and the focus of the *middle* (triple) three times. The passage of the blue and red rays at the extreme of the pencil is shown in contrast with the preceding, the separation from the same front being alike.

The inner and outer, or blue and red rays, after passing the first surface of the triple middle, meet the concaves of the flint, which refract the blue rays to a greater extent than the red, and cause them to converge (instead of diverging, as in the opposing half diagram), so that at their exit from the triple they meet and would cross, effecting what is known as "over-correction;" but this is so balanced and readjusted by the single back of crown glass, that the rays are finally united, and emerge in a state of parallelism. This form of object-glass is suitable for the high powers, or such as have a cover adjustment, viz. from the " $\frac{1}{8}$ -inch" upwards; perfect colour-correction is equally to be obtained in all of them.

It may be asked by some who have devoted their attention to the higher branches of optical mathematics, why the above result should have been worked out entirely by diagrams. But it has been found such a difficult task to calculate the passage of the two rays of greatest and least refrangibility through a combination having sixteen surfaces of glass of three different densities and refractions, that even first-class mathematicians have hitherto shrunk from the attempt.

Diagrams, however, are surprisingly accurate in their capability of indicating causes and results in the microscope and object-glass; for these lenses are minute, with deep curves and abrupt refractions; so that if the projection is worked out some fifty times the size of the original,

small errors can be detected. The work should be commenced at the back from a long conjugate focus, which, not being a constant distance, may be taken as very near to parallelism. The high powers all have the means of correction within this distance, and perform better with a long posterior focus than with a very short one. The relative indices for the two or more rays should be marked on a large pair of proportional compasses, the long limb representing the sine of the angle of incidence, and the short one that of refraction. Both the sines ought to be set off in the diagram *behind*, and neither of them in front of the ray in course of projection; this leaves the way clear, with the least confusion of lines.

At the same time a second or counterpart diagram should be at hand, to which the rays only are transferred as soon as their direction is ascertained; with these precautions a mistake is scarcely possible.

Now it is hoped that some improvements may be effected by this investigation, on account of the simplicity attained in the combination, in which we have *two single lenses* of crown, whose foci bear a definite proportion to each other; while all the corrections are performed by *one concave of dense flint*, the acting condition of which is not altered by the influence of any other concaves acting in the combination, and hitherto taking a share of the duty. This one flint is now to be considered singly as the heart and centre of the system in reference to the correction of the rays entering and leaving.

This memoir is of necessity incomplete, for want of definite information concerning the optical properties of various kinds of glass. Data obtained from working them into small lenses furnish only a rough approximation to the mean dispersive power of the combined flint and crown having the best apparent effect. Of the intermediate rays, little can be known beyond the mere appearance of more or less of a secondary spectrum.

Nothing of importance has been published since Fraunhofer's Table, containing the refractive indices for each of the seven primary colour-lines of the spectrum for ten kinds of glass: great advance has been effected since that date in the manufacture of optical glass, a most complete collection of which of every variety has been made by the Rosses up to the present date. Selected specimens from this will be worked into prisms, and the relative spectra mapped out by the Fraunhofer lines, leading, it is hoped, to the discovery of a combination of crown and flint glass which shall be free from secondary spectrum or absolutely achromatic. The result of this investigation will be the subject of a future communication.

II. "Note on an Erroneous Extension of Jacobi's Theorem." By
ISAAC TODHUNTER, M.A., F.R.S. Received October 25, 1872.

1. It is well known that Jacobi discovered the possibility of the relative equilibrium of a mass of homogeneous fluid which is in the form of an ellipsoid and rotates with uniform angular velocity round the least principal axis. A few days since, in reading over for the press a manuscript which had been written last year, I observed I had drawn attention to the circumstance that such relative equilibrium would be impossible if the ellipsoid rotated round any other straight line. Almost immediately afterwards I was accidentally glancing for the first time at the elaborate treatise on Mechanics published in 1870 by Dr. W. Schell, under the title of "*Theorie der Bewegung und der Kräfte*," and I noticed an account of Jacobi's theorem. Dr. Schell records that Jacobi was led to the discovery of his theorem by reason of an erroneous statement, made by Pontécoulant, that such a result was impossible; Jacobi undertook the inquiry, as he said, by virtue of a "certain spirit of contradiction to which he owed his most important discoveries" (see page 966 of Dr. Schell's volume). It should be remarked, however, that as to the error, Pontécoulant merely followed Lagrange.

2. I was much surprised to find that on the same page Dr. Schell made the following assertion:—"It has been lately shown by Dahlander that the relative equilibrium of the rotating ellipsoid will subsist even when the axis of rotation does not coincide with a principal axis of the ellipsoid." A reference is supplied to a memoir by Dahlander in Poggendorff's '*Annalen*,' vol. cxxix. (1866) p. 443. Notwithstanding this combination of authority the assertion is incorrect, as I shall now show.

3. I assume that when a mass of fluid is rotating with uniform angular velocity round a fixed axis, the problem of determining the pressure of the fluid and the form of the free surface may be changed from a dynamical form to a statical in the following manner:—Suppose the rotation stopped, and supply at every point an acceleration at right angles to the axis outwards from the axis, equal to $r\omega^2$, where ω is the angular velocity, and r is the distance of the point from the axis of rotation. This can be easily demonstrated, and is, in fact, always taken for granted by writers on the subject.

4. I will confine myself for simplicity to the case in which the assumed axis of rotation passes through the centre of the ellipsoid; but it will be easily seen that the process is applicable when this condition is not fulfilled. Suppose that an ellipsoid, of which the axes are $2a$, $2b$, $2c$, rotates about a diameter which makes with the principal axes angles whose direction cosines are l , m , n . Take for the equation to the ellipsoid

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1. \quad \dots \dots \dots (1)$$

Let O denote the centre of the ellipsoid, M any point of the mass whose coordinates are x, y, z , and N the point where the perpendicular from M on the axis of rotation meets that axis. Let ξ, η, ζ be the coordinates of N.

Then the acceleration $\omega^2 MN$, when resolved parallel to the axes, gives rise to the components

$$\omega^2(x-\xi), \quad \omega^2(y-\eta), \quad \omega^2(z-\zeta)$$

respectively. We must now determine ξ, η, ζ .

We know that

$$\cos \text{MON} = \frac{lx + my + nz}{\text{OM}};$$

hence $\text{OM} \cos \text{MON}$, that is $\text{ON} = lx + my + nz$. Denote this by v ; then we have

$$\xi = lv, \quad \eta = mv, \quad \zeta = nv.$$

The attractions of the ellipsoid at (x, y, z) parallel to the axes will be respectively

$$Px, \quad Qy, \quad Rz;$$

where P, Q, R are certain constants, in the form of definite integrals, which depend on the ratios of the axes of the ellipsoid.

Hence if p denote the pressure, and ρ the density of the fluid, we have

$$\frac{dp}{dx} = \rho \{ \omega^2(x - lv) - Px \},$$

$$\frac{dp}{dy} = \rho \{ \omega^2(y - mv) - Qy \},$$

$$\frac{dp}{dz} = \rho \{ \omega^2(z - nv) - Rz \}.$$

Therefore the form of the free surface will be determined by the equation

$$\omega^2(x^2 + y^2 + z^2) - \omega^2(lx + my + nz)^2 - Px^2 - Qy^2 - Rz^2 = \text{constant}. \quad (2)$$

By supposition, (1) and (2) must represent the same surface. But this is obviously impossible, unless two out of the three l, m, n vanish. Thus the axis of rotation must coincide with one of the principal axes; and then it follows in the known way that this must be the *least* principal axis.

5. Now let us turn to the memoir by Dahlander in Poggendorff's 'Annalen.' The process is this. Dahlander supposes that there are three simultaneous angular velocities, $\omega, \omega', \omega''$, round the axes of x, y, z respectively; and then he *assumes* the equation

$$\frac{dp}{\rho} = -(P - \omega'^2 - \omega''^2)x dx - (Q - \omega^2 - \omega''^2)y dy - (R - \omega^2 - \omega'^2)z dz.$$

This equation, however, is unjustifiable. Dahlander does not say how he obtained it, so that it is impossible to point out exactly where his error lies. Perhaps the equation was supposed to hold in virtue of some unwarranted extension of the principle in article 3. To show that the equation is

wrong, it is sufficient to observe that it makes $\frac{dp}{dx}$ involve only the

variable x ; but the three angular velocities are equivalent to a single angular velocity, and then, as we see in article 4, we shall have $\frac{dp}{dx}$ involving also y and z .

6. The problem which Dahlander discusses is in reality much simpler than his enunciation implies; it amounts to this: investigate the conditions under which a fluid will be in equilibrium in the form of an ellipsoid, when, besides the attraction of the fluid, there are forces parallel to the principal axes, which may be denoted by fx , gy , hz respectively. Traces of such a problem appear in other places—as, for example, in Lagrange's 'Mécanique Analytique,' première partie, Sect. VII. This is, however, different from the problem of *rotating* fluid, which it was proposed to discuss.

7. There is nothing to call for remark in the mathematical work of the memoir, except that a wrong value is assigned to the definite integral

$$\int_0^1 \frac{(1-u^2)u^2 du}{(1+\lambda^2 u^2)^{\frac{3}{2}}}. \quad \text{The correct value is}$$

$$\frac{3+2\lambda^2}{2\lambda^6} \log \{\lambda + \sqrt{(1+\lambda^2)}\} - \frac{3\sqrt{(1+\lambda^2)}}{2\lambda^4}.$$

October 24, 1872.

III. Additional Note to the Paper "On a supposed Alteration in the Amount of Astronomical Aberration of Light produced by the Passage of the Light through a considerable thickness of Refracting Medium." By the PRESIDENT. Received November 2, 1872.

Some months since I communicated to the Royal Society the result of observations on γ Draconis made with the water-telescope of the Royal Observatory (constructed expressly for testing the equality of the coefficient of sidereal aberration, whether the tube of a telescope be filled with air, as usual, or with water) in the spring and autumn of 1871. Similar observations have been made in the spring and autumn of 1872, and I now place before the Society the collected results. It will be remembered, from the explanation in the former paper, that the uniformity of results for the latitude of station necessarily proves the correctness of the coefficient of aberration employed in the Nautical Almanac.

Apparent Latitude of Station.

1871. Spring	51° 28' 34".4
Autumn	51 28 33.6
1872. Spring	51 28 33.6
Autumn	51 28 33.8

I now propose, when the risk of frost shall have passed away, to reverify the scale of the micrometer, and then to dismount the instrument.

IV. "Account of a Meteor that fell on the 'Seven Stones' Light-ship," in a Letter from the Secretary to the Corporation of the Trinity House, addressed to the PRESIDENT. Received January 9, 1873.

Trinity House, London, E.C., 9th January, 1873.

SIR,—I am directed to acquaint you that on the 13th of November last, at 2 A.M., a meteor burst against the 'Seven Stones' light-vessel belonging to this Corporation, and moored about $9\frac{1}{2}$ miles E. by N. of the Scilly Islands; and that it has been reported that the watch were struck senseless for a short period, seeing nothing before the shock, but that, on recovery, balls of fire like large stars were falling in the water like splendid fireworks, and that the decks were covered with cinders, which crushed under the sailors' feet as they walked. It appeared, the men said, as if something was passing swiftly and met with the obstruction of the vessel and burst.

The superintendent reports that the men say there was a very decided smell of brimstone, but add that they did not mention that until he asked them. There is reason to fear that the cinders were all washed off the decks by the rain and sea before daylight; and it happened also unfortunately that the men did not think to observe the compasses.

The Elder Brethren have great doubts whether any more accurate account of the phenomenon than the foregoing is obtainable; at the same time they direct me to report the matter to you, conjoined with the assurance that if any important or interesting scientific question is likely to be elucidated by further inquiry, they will have great pleasure in causing special questions to be put to the men, or in affording any member of the Royal Society facilities for investigation.

I am, Sir,

Your most humble Servant,

Sir G. B. Airy, K.C.B., &c. &c.

ROBIN ALLEN, *Secretary*.

January 23, 1873.

T. ARCHER HIRST, Ph.D., Vice-President, in the Chair.

The following communications were read:—

- I. "Contributions to the History of the Orcins.—No. III. Amido-derivatives of Orcin"*. By JOHN STENHOUSE, LL.D., F.R.S., &c. Received November 7, 1872.

In a paper published in March 1871†, I stated that I had made some experiments on the action of reducing agents on trinitro-orcinic acid and

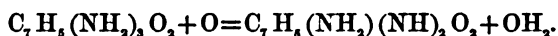
* A preliminary note on this subject appeared in the 'Chemical News,' vol. xxiii. p. 292, and Zeits. Chem. vol. vii. p. 414.

† Proc. Roy. Soc. vol. xix. p. 410.

trinitro-resorcinic acid; but as Dr. Schreder* has since published an account of some of the amido-derivatives of the latter substance, I have not pursued my investigations further in that direction, but confined myself to an examination of the products obtained from trinitro-orcinic acid.

Amido-diimido-orcin, $C_7H_5(NH_2)(NH)_2O_2$.—This compound, which has the properties of a base, is formed by the oxidation of triamido-orcin, and is most conveniently obtained in a pure state by decomposing a solution of the acetate with a slight excess of ammonia.

The most advantageous method of preparing the base is to reduce trinitro-orcin with sodium-amalgam, and to oxidize the alkaline solution of triamido-orcin by exposure to the air. The details of the process are as follows:—One part of trinitro-orcin and forty or fifty parts of water are placed in a bottle furnished with a caoutchouc cork, and fragments of sodium-amalgam containing about 3 per cent. of sodium are gradually introduced and agitated with the solution, which first acquires a brown colour from the formation of an intermediate amido-product, probably analogous to picramic acid, but becomes colourless as soon as the trinitro-orcin is completely reduced. The solution should be cooled from time to time, as considerable heat is generated during the reaction. When the solution containing triamido-orcin and sodic hydrate has become cold, it is poured into a large flask and agitated briskly; by this means the triamido-orcin is oxidized to amido-diimido-orcin and the solution assumes a magnificent blue colour:



A few seconds' agitation is sufficient, as if it be continued after the blue colour is fully developed, the amido-diimido-orcin in the strongly alkaline solution undergoes further oxidation and is destroyed. On strongly acidulating the blue solution with sulphuric or hydrochloric acid, the corresponding salt of amido-diimido-orcin is precipitated. Instead of directly agitating the colourless solution with air, it may be first neutralized or rendered very slightly acid with hydrochloric acid, cooled, and then rendered alkaline with ammonia. If this solution be now agitated in the manner before described it becomes filled with minute green needles of amido-diimido-orcin. Although the first method gives the best results when carefully conducted, it requires considerable experience to stop the oxidation at the precise point when the whole of the triamido-orcin is oxidized to amido-diimido-orcin, and before the latter becomes destroyed. With the ammoniacal solution there is far less danger of over oxidation.

Trinitro-orcin is also reduced by treatment with tin and hydrochloric acid, or zinc and hydrochloric or sulphuric acid. One part of trinitro-orcin and four parts of granulated tin are heated in a capacious flask with eight measures of concentrated hydrochloric acid diluted with sixteen measures of water. In a short time a powerful reaction takes

* Ann. Chem. Pharm. vol. clviii. p. 244.

place, so that it is advisable to remove the flask from the source of heat in order to prevent the contents from boiling over. When the action has become somewhat moderate, the solution is boiled until it is colourless, then diluted with water and the tin precipitated by hydrosulphuric acid. The clear solution acquires a purple tint on standing, and deposits large dark-coloured prisms of amido-diimido-orcin hydrochlorate, or the amido-diimido-orcin may be obtained directly by adding a slight excess of ammonia to the filtrate from the tin sulphide and oxidizing by agitation in the presence of air, when the base immediately separates in minute green needles. Trinitro-orcin and granulated zinc are boiled in a flask with twenty or thirty parts of water, and hydrochloric acid added in small quantities at a time until the solution becomes almost colourless. The clear liquid, poured from the excess of zinc and allowed to cool, is rendered slightly alkaline by ammonia and exposed to the air. As soon as the triamido-orcin is oxidized, which may be known by the brown colour of the product, an excess of hydrochloric acid is added to dissolve the zinc oxide and precipitate the amido-diimido-orcin as hydrochloride. The yield is, however, considerably less than when sodium-amalgam is employed as the reducing agent.

The amido-diimido-orcin sulphate or hydrochlorate obtained by any of the above processes is readily decomposed by treatment with a slight excess of dilute ammonia, leaving the free base in an impure state. This should then be dissolved in warm dilute acetic acid, filtered, and precipitated by a slight excess of ammonia. Two or three solutions and reprecipitations are sufficient to render it pure.

Pure amido-diimido-orcin crystallizes in small needles, which have a dark green metallic lustre by reflected light. They are insoluble in alcohol, ether, and benzol, and almost insoluble in water and dilute ammonia. Strong ammonia only dissolves the base in small quantity, yielding a pale blue solution, but it is readily soluble in a solution of sodic hydrate, with a fine deep blue colour; the solution, however, when boiled, loses its colour, ammonia being at the same time evolved. The base gives off ammonia when heated, and leaves a carbonaceous residue very difficult of combustion. As might be expected, when amido-diimido-orcin is treated with sodium-amalgam, it is reconverted into triamido-orcin.

Analysis of amido-diimido-orcin.—290 grm. substance, dried *in vacuo*, lost 0.025 grm. when dried at 100°, equivalent to 8.62 per cent.

I. 140 grm. substance, dried at 100°, gave 0.234 grm. carbonic anhydride and 0.075 grm. water.

II. 290 grm. substance, dried at 100°, gave 0.487 grm. carbonic anhydride and 0.152 grm. water.

		Theory.	I.	II.	Mean.
C ₇	= 84	= 45.40	45.59	45.77	45.68
H ₁₁	= 11	= 5.95	5.95	5.81	5.88
N ₃	= 42	= 22.70
O ₃	= 48	= 25.95
	<u>185</u>	<u>100.00</u>			

It will be seen that the results of the analyses agree with the formula $C_7(CH_3)(NH_2)(NH)_2(OH)_2 + H_2O$ for the substance dried at 100° , and the formula for the substance dried *in vacuo* is probably



as it requires 8.87 per cent. water.

Triamido-orcin.—The colourless solution obtained by the reduction of trinitro-orcin with tin and hydrochloric acid, after removal of the tin by sulphydric acid, appears to contain triamido-orcin hydrochloride along with excess of hydrochloric acid. On concentrating the solution at 100° much of the triamido-orcin is decomposed and a considerable amount of ammonium chloride formed; whilst, although long needles of triamido-orcin hydrochloride are obtained by evaporating it in a vacuum, yet, owing to their great solubility and the readiness with which they absorb water and deliquesce, the salt has not yet been obtained in a state fit for analysis. On moistening the crystals of this hydrochloride with ammonia, they become almost instantaneously converted into the metallic green needles of the amido-diimido-orcin.

If a current of sulphuretted hydrogen be passed through a solution of ammonium sulphhydrate in which amido-diimido-orcin is suspended, the latter rapidly loses its colour and becomes converted into a sandy deposit consisting of colourless crystals. These are apparently triamido-orcin, and may be washed by decantation with a dilute solution of ammonium sulphhydrate, in which they are but slightly soluble. These crystals rapidly acquire the metallic green lustre of amido-diimido-orcin when exposed to the air, and are readily soluble in dilute acids. The hydrochloric-acid solution behaves in a manner precisely similar to that obtained by the reduction of trinitro-orcin with tin and hydrochloric acid, becoming deep red, and depositing crystals of amido-diimido-orcin hydrochloride when exposed to the air.

Amido-diimido-orcin hydrochloride.—The hydrochloride obtained in the preparation of amido-diimido-orcin, as described in the earlier part of this communication, may be purified by crystallization from hot water; but as heat decomposes solutions of the salts of this base, it is better to precipitate a cold solution of the acetate by a slight excess of hydrochloric acid, in which the hydrochloride is but slightly soluble: the precipitate should be thoroughly washed with alcohol, pressed, and dried.

Pure amido-diimido-orcin hydrochloride crystallizes in different ways, according to the circumstances under which the crystals are formed. As

produced directly by adding hydrochloric acid to the blue solution of the base in caustic soda obtained from trinitro-orcin by sodium-amalgam, it forms long silky needles of a brownish-red colour: an aqueous solution of the acetate or hydrochloride precipitated by an excess of hydrochloric acid yields a mixture of these needles with rhomboidal plates; the latter are purple by reflected light, and of an olive-green colour by transmitted light. The slow oxidation of the hydrochloric-acid solution of triamido-orcin obtained by means of tin and hydrochloric acid yields dark-coloured short thick prisms. The hydrochloride is insoluble in alcohol and ether, moderately soluble in cold water, and readily in boiling water, although the latter causes partial decomposition. Its aqueous solution is precipitated almost entirely on acidulating it with hydrochloric acid; but the salt is soluble in concentrated hydrochloric acid, especially when warm, forming a purple solution. On boiling this the salt is rapidly decomposed and the colour changes to a dirty green.

Analysis of amido-diimido-orcin hydrochloride.—428 grm. substance, dried *in vacuo*, lost .035 grm. when heated to 100°, corresponding to 8.41 per cent. water.

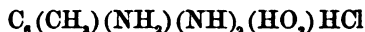
I. .255 grm. substance, dried at 100°, gave .180 grm. argentic chloride.

II. .232 grm. substance, dried at 100°, gave .164 grm. argentic chloride.

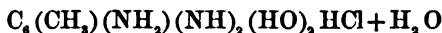
III. .344 grm. substance, dried at 100°, gave .242 grm. argentic chloride.

	Theory.	I.	II.	III.	Mean.
$C_6H_{10}N_3O_2 =$	168	82.55			
$Cl =$	35.5	17.45	17.47	17.49	17.40
	203.5	100.00			

These results correspond nearly to the formula



for the substance dried at 100°; and the formula



requires 8.13 per cent. water, and would therefore appear to be that of the substance dried *in vacuo*.

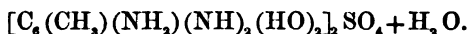
Amido-diimido-orcin sulphate.—This salt is readily prepared by precipitating a dilute solution of the acetate with sulphuric acid, when it forms minute lustrous plates which are purple by reflected light. By slow crystallization from a hot aqueous solution it may be obtained in large leaf-like plates. It somewhat resembles the hydrochloride in its properties, but is much less soluble in water.

Analysis of amido-diimido-orcin sulphate.—I. .255 grm. substance, dried at 100°, gave .128 grm. barium sulphate.

II. .284 grm. substance, dried at 100°, gave .143 grm. barium sulphate.

	Theory.	I.	II.	Mean.
$(C_7H_{12}N_2O_2)_2 = 372$	79.49
$SO_4 = 96$	20.51	20.65	20.72	20.69
	<hr/> 468	<hr/> 100.00		

The formula of this salt would therefore appear to be



Amido-diimido-orscin nitrate is prepared, like the sulphate, by adding a slight excess of nitric acid to a moderately strong solution of the acetate and washing the precipitate with alcohol. It closely resembles the sulphate in appearance, but is much more soluble in water. When heated with excess of nitric acid it is decomposed, yielding a yellow solution, which, on being evaporated, leaves a mixture of oxalic acid and an amorphous yellow substance.

Amido-diimido-orscin acetate.—Amido-diimido-orscin dissolves readily in acetic acid; and on carefully evaporating the solution at a low temperature, the acetate is obtained in ill-defined crystalline plates having a purple iridescence. It is readily soluble in cold water, but only slightly soluble in glacial acetic acid.

Amido-diimido-orscin oxalate.—Very slightly soluble purple scales obtained by precipitating a solution of the acetate with oxalic acid.

Amido-diimido-orscin picrate.—On adding a solution of picric acid to a dilute solution of amido-diimido-orscin acetate and washing the precipitate with alcohol, the picrate is obtained in iridescent green needles and plates. It is insoluble in alcohol, and but slightly soluble in water.

I cannot conclude this paper without acknowledging the very efficient aid I have received from my assistant, Mr. Charles Edward Groves, in conducting this investigation.

II. "Note on the Wide-slit Method of viewing the Solar Prominences." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received November 21, 1872.

When editing the English translation of Schellen's 'Spectrum Analysis,' I discovered that the short account of the method of viewing the forms of the solar prominences by means of a wide slit, which I had the honour of presenting to the Royal Society on February 16, 1869*, does not agree exactly in one respect with the account of the observation of February 13 as it was entered at the time in my observatory book. The short note was written at the suggestion of a friend during a Committee held in the Royal Society's Apartments, and, as the concluding words show, was intended to be followed by a more detailed account of the method of observation. The point in question relates to the position of a second slit which was used to screen the eye from every part of the

* Proc. Roy. Soc. vol. xvii. p. 302.

spectrum except that under observation. The words in my book written at the time are, "narrow slit found to be best at focus of little telescope with positive eyepiece." In the note the second slit was stated to have been placed before the object-glass of the little telescope. Such an arrangement was tried in connexion with some other experiments in progress at the time. The plan of limiting the field of view to the part of the spectrum corresponding to the refrangibility of the light of the prominence, as well as the employment of a ruby glass, is of value when the air is not favourable, or when a spectroscope of small dispersive power is used.

III. "On the Fossil Mammals of Australia.—Part VIII. Family MACROPODIDÆ: Genera *Macropus*, *Osphranter*, *Phascolagus*, *Sthenurus*, and *Protemnodon*." By Professor OWEN, F.R.S. &c. Received November 11, 1872.

(Abstract.)

In the present Part of the series of papers on the Fossil Mammals of Australia, the author enters upon the description and determination of the fossils referable to the family of Kangaroos (Macropodidæ), restricting, however, the latter term to the species in which the molar teeth have two transverse ridges for the chief character of their grinding-surface, and excluding the Potoroos (Hypsiprymniidæ), in which the working-surface of the molars is formed by four tubercles in two transverse pairs.

The large extinct species of Kangaroo indicated under the names *Macropus Titan*, *M. Atlas*, and *M. Anak* in former publications ('Mitchell's Three Expeditions into the Interior of Eastern Australia,' 2 vols. 8vo, 1838, Palæontological Appendix, vol. ii. p. 59, pls. 24-32; also 'Proceedings of the Geological Society of London,' vol. xv. p. 185, 1858) here receive further elucidation of their specific distinction from any known living Kangaroos and of the grounds (according to the value assigned thereto by present zoologists) for referring two of these (*M. Atlas* and *M. Anak*) to distinct subgenera of Macropodidæ.

As the extinct Kangaroos which the present paper defines are chiefly represented by fossil jaws and teeth, some remarks on the dentition of existing Kangaroos, with requisite illustrations, are premised, and the parts of the complex molars are defined.

The author then enters on the elucidation, aided by the facts premised, of *Macropus Titan*, *M. affinis*, *Osphranter Cooperi*, *O. Gouldii*, *Phascolagus altus*, *Sthenurus Atlas*, *S. Brehus*, *Protemnodon Anak*, *P. Og*, *P. Mimas*, and *P. Ræchus*.

The maxillary, mandibular, and dental characters of these extinct species are illustrated by the subjects of eight Plates.

January 30, 1873.

GEORGE BUSK, Esq., Vice-President, in the Chair.

The following communications were read :—

- I. "Note on the Origin of *Bacteria*, and on their Relation to the Process of Putrefaction." By H. CHARLTON BASTIAN, M.D., F.R.S. Received November 20, 1872.

In his now celebrated memoir of 1862, M. Pasteur asserted and claimed to have proved (1) that the putrefaction occurring in certain previously boiled fluids after exposure to the air was due to the contamination of the fluids by *Bacteria*, or their germs, which had before existed in the atmosphere, and (2) that all the organisms found in such fluids have been derived more or less immediately from the reproduction of germs which formerly existed in the atmosphere.

The results of a long series of experiments have convinced me that both these views are untenable.

In the first place, it can be easily shown that living *Bacteria*, or their germs, exist very sparingly in the atmosphere, and that solutions capable of putrefying are not commonly infected from this source.

It has now been very definitely ascertained that certain fluids exist which, after they have been boiled, are incapable of giving birth to *Bacteria*, although they continue to be quite suitable for the support and active multiplication of any such organisms as may have been purposely added to them. Amongst such fluids I may name that now commonly known as "Pasteur's solution," and also one which I have myself more commonly used, consisting of a simple aqueous solution of neutral ammoniac tartrate and neutral sodic sulphate*. When portions of either of these fluids are boiled and poured into superheated flasks, they will continue quite clear for many days, or even for weeks—that is to say, although the short and rather narrow neck of the flask remains open the fluids will not become turbid, and no *Bacteria* are to be discovered when they are submitted to microscopical examination.

But in order to show that such fluids are still thoroughly favourable media for the multiplication of *Bacteria*, all that is necessary is to bring either of them into contact with a glass rod previously dipped into a fluid containing such organisms. In about thirty-six hours after this has been done (the temperature being about 80° F.), the fluid, which had hitherto remained clear, becomes quite turbid, and is found, on examination with the microscope, to be swarming with *Bacteria*†.

Facts of the same kind have also been shown by Dr. Burdon Sander-

* In the proportion of 10 grains of the former and 3 of the latter to 1 ounce of distilled water.

† The Modes of Origin of Lowest Organisms, 1871, pp. 30, 51.

son* to hold good for portions of boiled "Pasteur's solution." Air was even drawn through such a fluid daily for a time, and yet it continued free from *Bacteria*.

Evidence of this kind has already been widely accepted as justifying the conclusion that living *Bacteria* or their germs are either wholly absent from or, at most, only very sparingly distributed through the atmosphere. The danger of infection from the atmosphere having thus been got rid of and shown to be delusive, I am now able to bring forward other evidence tending to show that the first *Bacteria* which appear in many boiled infusions (when they subsequently undergo putrefactive changes) are evolved *de novo* in the fluids themselves. These experiments are moreover so simple, and may be so easily repeated, that the evidence which they are capable of supplying lies within the reach of all.

That boiling the experimental fluid destroys the life of any *Bacteria* or *Bacteria*-germs preexisting therein is now almost universally admitted; it may, moreover, be easily demonstrated. If a portion of "Pasteur's solution" be purposely infected with living *Bacteria* and subsequently boiled for two or three minutes, it will continue (if left in the same flask) clear for an indefinite period; whilst a similarly infected portion of the same fluid, not subsequently boiled, will rapidly become turbid. Precisely similar phenomena occur when we operate with the neutral fluid which I have previously mentioned; and yet M. Pasteur has ventured to assert that the germs of *Bacteria* are not destroyed in neutral or slightly alkaline fluids which have been merely raised to the boiling-point†.

Even M. Pasteur, however, admits that the germs of *Bacteria* and other allied organisms are killed in slightly acid fluids which have been boiled for a few minutes; so that there is a perfect unanimity of opinion (amongst those best qualified to judge) as to the destructive effects of a heat of 212° F. upon any *Bacteria* or *Bacteria*-germs which such fluids may contain.

Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask with the assurance that it contains no living organisms. Having ascertained also by our previous experiments with the boiled saline fluids that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion invariably becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*.

Thus if we take two similar flasks, one of which contains a boiled "Pasteur's solution" and the other a boiled turnip-infusion, and if we

* Thirteenth Report of the Medical Officer of the Privy Council (1871), p. 59.

† How unwarrantable such a conclusion appears to be, I have elsewhere endeavoured to show. See 'Beginnings of Life,' 1872, vol. i. pp. 326-333, 372-399.

place them beneath the same bell-jar, it will be found that the first fluid remains clear and free from *Bacteria* for an indefinite period, whilst the second invariably becomes turbid in one or two days.

What is the explanation of these discordant results? We have a right to infer that all preexisting life has been destroyed in each of the fluids*; we have proved also that such fluids are not usually infected by *Bacteria* derived from the air; in this very case, in fact, the putrescible saline fluid remains pure, although the organic infusion standing by its side rapidly putrefies. We can only infer, therefore, that whilst the boiled saline solution is quite incapable of engendering *Bacteria*†, such organisms are able to arise *de novo* in the boiled organic infusion.

Although this inference may be legitimately drawn from such experiments as I have here referred to, fortunately it is confirmed and strengthened by the labours of many investigators who have worked under the influence of much more stringent conditions, and in which closed vessels of various kinds have been employed‡.

Whilst we may therefore infer (1) that the putrefaction which occurs in many previously boiled fluids when exposed to the air is not due to a contamination by germs derived from the atmosphere, we have also the same right to conclude (2) that in many cases the first organisms which appear in such fluids have arisen *de novo*, rather than by any process of reproduction from preexisting forms of life.

Admitting, therefore, that *Bacteria* are ferments capable of initiating putrefactive changes, I am a firm believer also in the existence of not-living ferments under the influence of which putrefactive changes may be initiated in certain fluids—changes which are almost invariably accompanied by a new birth of living particles capable of rapidly developing into *Bacteria*.

II. "On Just Intonation in Music; with a description of a new Instrument for the easy control of all Systems of Tuning other than the ordinary equal Temperament." By R. H. M. BOSANQUET, Fellow of St. John's College, Oxford. Communicated by Professor H. J. S. SMITH, F.R.S., Savilian Professor of Geometry in the University of Oxford.

(Abstract.)

The object of this communication is to place the improved systems of

* [Note. Jan. 31, 1873.]—In 'The Beginnings of Life,' vol. i. p. 332, note 1, I have cited facts strongly tending to show that *Bacteria* are killed in infusions of turnip or of hay, when these have been heated to a temperature of 140° F. They also seem to die at the same temperature in solutions of ammoniac tartrate with sodic phosphate.

† See 'Beginnings of Life,' vol. ii. p. 35, and vol. i. p. 463.

‡ See a recent communication by Prof. Burdon Sanderson, in 'Nature,' January 9th.

tuning within the reach of ordinary musicians; for this purpose the theory and practice are reduced to their simplest forms.

A notation is described, adapted to use with ordinary written music, by which the notes to be performed are clearly distinguished.

The design of a key-board is described, by which any system of tuning, except the ordinary equal temperament, can be controlled, if only the fifths of the system be all equal. The design is on a symmetrical principle, so that all passages and combinations of notes are performed with the same handling, in whatever key they occur.

The theory of the construction of scales is then developed, and a diagram is given, from which the characteristics of any required system can be ascertained by inspection.

An account is then given of the application of such systems to the new key-board, and particularly of an harmonium which has been constructed and contains at present the division of the octave into fifty-three equal intervals in a complete form. Rules for tuning are given.

Finally, the application of the system of fifty-three to the violin is discussed.

Throughout the work of former labourers in the same field is reviewed: the obligations of the writer are due to Helmholtz, the late General T. Perronet Thompson, F.R.S., and others.

III. "On the Composition and Origin of the Waters of a Salt Spring in Huel Seton Mine, with a Chemical and Microscopical examination of certain Rocks in its vicinity." By J. ARTHUR PHILLIPS, Mem.Inst.C.E. Communicated by Professor RAMSAY, F.R.S. Received December 12, 1872.

(Abstract.)

Huel Seton Copper-Mine is situated about one mile north-east of the town of Camborne, Cornwall, and is distant from the sea, on the north coast, a little more than three miles.

The workings of Huel Seton are entirely in "killas," or clay-slate, and the saline waters issue at the rate of 50 gallons per minute, and at a temperature of 92° F., from the eastern fore breast of the 160-fathom level. This has intersected a fault, or cross course, which may be traced in a northerly direction to the sea. The temperature of the level from the end of which the water issues, like that of the water itself, is 92° F. The following results, in grammes per litre and grains per gallon, were obtained by analysis. Sp. gr. 1.0123. Total solid contents 14.3658 grammes per litre, or 1005.61 grains per gallon.

	Grammes per litre.		Grains per gallon.	
	I.	II.	I.	II.
Carbonic acid	·0795	·0786	5·56	5·50
Sulphuric acid	·0178	·0177	1·25	1·24
Silica	·0270	·0280	1·89	1·96
Chlorine	9·1728	9·1662	642·10	641·63
Bromine	trace.	trace.	trace.	trace.
Alumina	·3456	·3460	24·19	24·22
Ferric oxide	·0031	·0033	·22	·23
Manganese	trace.	trace.	trace.	trace.
Copper	minute trace.	minute trace.	minute trace.	minute trace.
Lime	3·4795	3·4963	243·56	244·74
Magnesia	·0721	·0710	5·05	4·97
Alkaline chlorides	6·4920	6·4626	454·44	452·38
Potassium	·0832	·0835	5·82	5·84
Cæsium*	trace.	trace.	trace.	trace.
Sodium	2·2977	2·2885	160·84	160·19
Lithium	·0805	·0794	5·63	5·56
Ammonia	trace.	trace.	trace.	trace.
Nitric acid	trace.	trace.	trace.	trace.

The foregoing results may be thus tabulated † :—

	Grammes per litre.		Grains per gallon.	
	I.	II.	I.	II.
Calcium carbonate.....	·0921	·1011	6·45	7·08
Ferrous carbonate	·0045	·0047	·31	·33
Manganous carbonate	trace.	trace.	trace.	trace.
Calcium sulphate	·0303	·0301	2·12	2·11
Cupric chloride	minute trace.	minute trace.	minute trace.	minute trace.
Calcium chloride	6·7697	6·7934	473·88	475·54
Magnesium chloride	·1712	·1686	11·98	11·80
Aluminium chloride	·9003	·9013	63·02	63·09
Potassium chloride.....	·0919	·0900	6·43	6·30
Calcium chloride	trace.	trace.	trace.	trace.
Sodium chloride.....	5·8442	5·8210	409·09	407·47
Lithium chloride	·4888	·4820	34·22	33·74
Potassium bromide	trace.	trace.	trace.	trace.
Potassium silicate (K_2SiO_3)	·0693	·0719	4·85	5·03
Nitric acid	trace.	trace.	trace.	trace.
Ammonia	trace.	trace.	trace.	trace.
Total found by addition of constituents	14·4623	14·4641	1012·35	1012·49
Total found directly ‡.....	14·3658	...	1005·61	...
Free carbonic acid.....	·0373	·0323	2·61	2·26

* The amount of cæsium appears to be very small. On adding chloride of platinum to a rather dilute solution of the alkaline chlorides obtained from this water, a slight yellow precipitate was deposited; this, after re-solution and the removal of the platinum by sulphuretted hydrogen, afforded by the spectroscope somewhat faint indications of the presence of cæsium.

† As the state of combination in which the various substances present in mineral waters exist cannot be accurately determined, the system of grouping adopted in the Table must to some extent be regarded as arbitrary.

‡ The difference between the amount of total solid contents found directly and that

A consideration of the various phenomena connected with the occurrence of this and other apparently similar springs which have at different times been discovered in the district, would seem to lead to the inference that they all have some more or less direct communication with the sea, and that they are either the result of infiltration of sea-water through faults, or are true and independent sources which, before being tapped below the sea-level, had found their way to the ocean through faults or channels.

The following would appear, in the present state of our knowledge, a not improbable explanation of the origin of the Huel Seton spring.

The cross course is believed to extend through both granite and clay-slate to the sea. From the close contact of its surfaces, the presence of clay, and from other causes, this fault may be supposed not to be uniformly permeable by water, which can only follow a circuitous passage. In this way it penetrates to depths where reactions take place, which, although not entirely in accordance with the results of daily experience in our laboratories, can, after the investigations of M. Daubrée, M. de Sénarmont, and others, be readily understood.

By the action of *sea-water* on *silicates of calcium*, *silicates of sodium* and *chloride of calcium* may be produced. The *sulphate of sodium* of the sea-water will be decomposed by this *chloride of calcium*, with the production of *sulphate of calcium* and *chloride of sodium*. The decomposition of clayey matter by *common salt* may produce *chloride of aluminium* and *silicates of sodium*, while the *magnesium* of the chloride of magnesium may be replaced by *calcium*; lastly, a portion of the *potassium* in the sea-water appears to have been replaced by the *lithium* of the granite.

Presents received January 9, 1873.

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obtained by addition of constituents, is doubtless in a great measure due to the partial decomposition of aluminium and magnesium chlorides at the temperature (180° C.) at which the drying of the residue was effected.

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"On some Properties of Anhydrous Liquefied Ammonia." By G. GORE, F.R.S. Received May 15, 1872*.

To examine the general solvent properties of anhydrous liquefied ammonia, and to detect any manifest chemical reactions between it and various substances, I employed precisely the same kind of method as that I used in examining liquid cyanogen (see *Proc. Roy. Soc.* vol. xx. p. 67).

It was desirable in these experiments not to use flint-glass tubes, because the tenacity of that substance is so feeble the tubes burst by the pressure of the gutta-percha stoppers; when such tubes were employed, the mouths of them had to be closed by fusion. The tubes require to be somewhat stronger than those used with liquid cyanogen, because the pressure exerted by the vapour is so much greater. The length of the horizontal portion of the tubes was $7\frac{1}{2}$ inches; by having this part of the tube very much longer, a much lower degree of heat is necessary in the experiments, and a greater quantity of the liquid may be obtained.

To procure the anhydrous liquid, chloride of calcium was heated to redness, reduced to fine powder, and perfectly saturated with well-dried ammonia in a current of that vapour, the flask containing the powder being immersed in broken ice during the latter part of the process, and occasionally shaken to prevent formation of lumps. During this process the powder evolved much heat and swelled considerably. Every precaution was taken to prevent the powder absorbing moisture from the atmosphere.

The temperature employed to expel the vapour from the salt, with tubes of the dimensions given, was much below that of visible redness. On allowing the tubes to become quite cold, the vapour was reabsorbed and the liquid disappeared; the pressure having also ceased, enabled the stopper to be removed and the products further examined. By this means some of the tubes could also be employed for fresh substances.

250 substances, including solids and liquids, and a great variety of simple and compound bodies, were subjected to the action of the liquefied vapour. Except when otherwise stated, the substances were in nearly all cases as free from moisture as it was convenient to obtain them; nevertheless in some instances the effects were evidently due in a slight degree to traces of water. The substances were also generally very pure, many of them having been carefully prepared by myself. The following is a statement of the results:—

Water mixed perfectly with the liquid. After the reabsorption of the ammonia by the chloride of calcium and the tube had become perfectly cold, the water had become about twice its original bulk by retention of ammonia.

Iodine first liquefied to the appearance of thin treacle, then entirely dissolved to a clear and slightly yellow liquid. On evaporation of the

* Read June 20, 1872. See abstract, vol. xx. p. 441.

ammonia, reddish-black crystals separated, apparently consisting of unaltered iodine. I did not examine whether the residue contained any of the explosive compound, "iodide of nitrogen." Iodic acid did not dissolve or show any change in the liquid.

Wood-charcoal, also Brodie's intumescent graphite, did not dissolve or change. C_2Cl_4 would not mix with the ammonia; but a little of it dissolved, and the ammonia became slightly brown. Liquid C_2Cl_4 mixed perfectly, and after the evaporation of the ammonia it was left behind. Solid C_2Cl_4 did not dissolve or suffer any change. Liquid CCl_4 behaved like C_2Cl_4 . Solid CBr_4 * dissolved rapidly and very copiously, and did not recrystallize until nearly all the liquid had evaporated.

Boron crystals, boracic anhydride, silicon crystals, and precipitated silica were all unaffected.

Sulphur dissolved sparingly, and formed a deeply coloured purplish-red solution, and on evaporation of the liquid a yellow film of it was left. Liquid chloride of sulphur became dark red and solid in the gas, and purple in the liquid, apparently with chemical action; a portion of the solid dissolved and formed a deeply purple liquid, on evaporation of which the red solid was left behind. Bisulphide of carbon became yellow and opaque in the gas, and changed in the liquid to a yellow bulky solid. Selenium was unaffected. Selenious acid prevented the formation of the liquid, probably by uniting with the ammonia, but did not enlarge in bulk, or show any visible change. Bright tellurium was unaffected.

Red phosphorus showed no change. White phosphorus dissolved to a yellow liquid, and separated again on evaporation of the ammonia. Phosphoric anhydride contracted in bulk in the gas, dissolved but very slightly in the liquid, and exhibited no other change. Glacial phosphoric acid was not visibly affected. Terchloride of phosphorus produced strong chemical action, and formed a colourless bulky solid. Pentachloride of phosphorus dissolved freely, and crystals formed on evaporation of the liquid.

Metallic arsenic was unaffected. Arsenious anhydride enlarged somewhat in bulk, but did not dissolve. Arsenic-acid crystals dissolved to a very minute extent only. Terchloride of arsenic was converted into a white solid with manifest chemical action. Teriodide of arsenic became white and swelled considerably. Realgar became yellow, and partly dissolved to a yellow solution, which on evaporation left an orange-yellow residue. Orpiment turned dark brown in the gas, and dissolved slightly in the liquid, producing a faintly yellow solution, which left a yellow film on evaporation.

Metallic antimony, sesquioxide of antimony, and antimonious acid were not visibly affected. Hydrated antimonious acid was slightly dissolved. Fused fluoride of antimony swelled very greatly in the liquid, but did not dissolve; it retained its enlarged bulk after evaporation of all the

* Given to me by the discoverers, Messrs. Groves and Bolas.

ammonia. Crystalline hydrated terchloride of antimony swelled considerably, but did not dissolve. Oxychloride of antimony was not visibly affected. Terbromide of antimony dissolved freely. Oxybromide of antimony was very slightly dissolved. Teriodide of antimony became yellow with apparent chemical action, and a portion dissolved and formed a dense layer beneath the colourless liquid ammonia. Black and red sesquisulphides of antimony were not visibly affected, but each imparted a faint purplish-blue colour to the liquid. Orange sulphide of antimony dissolved to some extent, forming an opaque brownish-green solution, and separated again on evaporation of the liquid. Vanadic acid and nitride of vanadium produced no visible effect*. "Molybdenum" (in the state of a fine black powder) imparted a weak cobalt-blue colour to the liquid, but did not visibly diminish in bulk. Molybdic acid became quite white, but did not dissolve. Native sulphide of molybdenum, "tungsten" (in the state of a black powder), and tungstic acid produced no visible effect.

Metallic bismuth, hydrated oxide of bismuth, and fluoride of bismuth were not visibly affected. Chloride of bismuth (not anhydrous) swelled greatly, and dissolved rather freely, but partly returned to its original bulk after the liquid had evaporated. Carbonate of bismuth and sulphide of bismuth were not visibly altered.

Metallic osmium was not visibly affected. Osmic acid was chemically altered, and produced first a yellow, then a red, and finally a brownish-black opaque mixture, consisting of solid particles suspended in a comparatively colourless liquid: on evaporation of the liquid, the solid substance remained behind. Black oxide of iridium, also scales of osmiridium, produced no visible effect. Metallic palladium was not affected. Bichloride of palladium became white, and enlarged in bulk, and slightly dissolved to a clear yellow liquid; the residue remained white and of enlarged bulk after evaporation of the liquid. Sulphide of palladium was unaffected. Black oxide of platinum was not visibly affected. Bichloride of platinum became nearly white, and slightly dissolved, forming a slightly yellow solution. Tetrachloride of platinum behaved like the bichloride. Tetraiodide of platinum turned red, dissolved freely to a reddish-yellow liquid, crystallized in red needles on evaporation of the solution, and finally left a black residue. Terchloride of gold was decomposed, and the product very slightly dissolved.

Bright silver was unaffected. Oxide of silver swelled considerably, but did not dissolve; it contracted again after evaporation of the liquid. Peroxide of silver (formed by electrolysis of a pure solution of argentic fluoride with platinum electrodes) became brown, and made the liquid slightly yellow, but did not visibly alter in bulk. Argentic nitrate dissolved freely, and on evaporation of the liquid long crystalline needles separated. Brown argentic fluoride swelled a little, became somewhat

* Given to me by Dr. Roscoe.

white, dissolved sparingly, and separated in needles on evaporation. Chloride of silver in powder swelled considerably, but did not dissolve; it remained enlarged in bulk after evaporation of the liquid. Fused argentic bromide lost its transparency, swelled and became white, dissolved slightly, and crystallized on evaporation. Argentic iodide dissolved rapidly and freely, and formed a dense colourless solution which mixed with the excess of liquefied ammonia; colourless crystals separated, but not until nearly all the liquid had evaporated: this salt is extremely soluble in the liquid. Argentic iodate showed no effect. Carbonate of silver turned dark brown and swelled considerably, but did not dissolve, and on evaporation of the liquid returned to its original volume and colour. Argentic sulphate swelled greatly, and disintegrated to a fine white powder, which returned partly to its original bulk after evaporation of all the liquid. Phosphate of silver and argentic arseniate were nearly insoluble, and suffered no chemical change. Vanadate of silver was insoluble.

Mercury was unaffected. Yellow mercuric oxide, also red-brown mercuric oxide (both obtained by precipitation), did not dissolve or alter. Mercurous nitrate turned black in the gas, and dissolved to some extent in the liquid; the residue left after evaporation of the liquid was a blackish-grey powder, apparently consisting of minute particles of metallic mercury. Mercurous chloride became brown in the gas, and dissolved copiously to a dense solution in the liquid; but its solution would not mix with the excess of liquefied ammonia: by evaporation of the ammonia it was left in a crystalline state. Mercuric chloride dissolved freely, forming a colourless dense solution, which would not mix or dissolve in the slightest degree in the excess of liquid: by evaporation the excess of liquid separated first, then that containing the chloride, and left the salt in a crystalline state. Mercuric bromide fell to powder with apparent chemical action, and dissolved to a small extent. Scarlet mercuric iodide first became yellow, then dark brown, and dissolved freely: on evaporation of the liquid, the salt separated in a crystalline state. Mercuric sulphide was unaffected. Anhydrous mercuric sulphate was very little affected. Hydrated mercuric sulphate swelled greatly, imparted a violet colour to the liquid, and partly returned to its former bulk after evaporation of the ammonia.

Copper lost its brightness, and imparted a slightly brown colour to the ammonia: when nearly all the liquid had evaporated, the residuary portion was deep blue in colour. Copper filings produced no additional effect. Suboxide of copper was nearly insoluble, and but little affected. Protoxide of copper was also insoluble and unaffected. White anhydrous cupric fluoride became deep blue in colour, but imparted scarcely any colour to the liquid. Anhydrous cupric chloride became deep blue, swelled greatly, did not dissolve nor impart colour to the liquid. Cuprous iodide (not perfectly anhydrous) became partly green, and imparted a

faint blue colour to the liquid, but did not otherwise dissolve. Cupric carbonate produced no effect. Cupric sulphide was superficially changed to a deep blue colour. White anhydrous cupric sulphate turned deep blue and swelled greatly, but did not impart any colour to the liquid; it remained enlarged in bulk after evaporation of the ammonia. Cupric phosphate produced no visible effect.

Nickel filings were unaffected. Anhydrous oxide of nickel and hydrated oxide of nickel were also unaffected. Fluoride of nickel suffered no great visible change, and did not dissolve. Nearly anhydrous chloride of nickel swelled and became of a purplish-blue colour, but did not dissolve: after evaporation of the liquid, the salt remained enlarged in bulk.

Oxide of cobalt, also fluoride of cobalt, was unaffected. Anhydrous chloride of cobalt swelled to about ten times its original volume and fell to powder, but was quite insoluble; it partly contracted in bulk again on evaporation of the ammonia. Carbonate of cobalt was unaffected. Sulphate of cobalt (not perfectly anhydrous) swelled, but did not dissolve; it remained expanded after evaporation of the liquid.

Electro-deposited iron was unaffected. Anhydrous persulphate of iron absorbed much of the liquid, swelled considerably, and showed signs of decomposition, but did not dissolve; it remained enlarged in bulk after evaporation of the liquid.

Metallic manganese, fluoride of manganese, chloride of manganese, and manganic sulphate were all unaffected.

Chromic acid turned yellow, absorbed ammonia, swelled greatly, dissolved slightly, forming a yellow solution, and retained its enlarged bulk after evaporation of the liquid. Solid green fluoride of chromium was not affected. Violet chloride of chromium swelled considerably and dissolved slightly, forming a purplish solution; it did not contract to its original bulk after evaporation of the liquid. Chromate of silver turned bright yellow at its edges, and nearly black in its mass, but did not dissolve. Chromate of copper was not dissolved, but was converted externally into a substance having the colour of sesquioxide of chromium. Metallic aluminium was unaffected. Yellow oxide of uranium and nitrate of uranium produced but little effect. Fluoride of uranium was slightly dissolved, forming a yellowish solution.

Metallic thallium, peroxide of thallium, and fluoride of thallium were not visibly altered. Bright metallic lead and red oxide of lead were unaffected. Nitrate of lead dissolved, and was apparently partly decomposed; crystals separated on evaporation of the last portions only of the liquid. Fluoride of lead produced no effect. Chloride of lead swelled considerably, but did not dissolve; it did not diminish in bulk on evaporation of the liquid. Iodide of lead turned white, dissolved slightly, and colourless crystals were left on evaporation of the liquid. Iodate of lead produced no effect. Yellow chromate of lead behaved in all respects like the iodide.

Metallic tin, stannous oxide, and stannic oxide were not affected. Fused bichloride of tin was partly dissolved. Liquid tetrachloride of tin was first converted into a solid substance with strong chemical action; the solid then dissolved freely, and separated again on evaporation of the liquid. Metallic indium was unaffected.

Fluoride of cadmium was not affected. Chloride of cadmium swelled, but did not dissolve; it contracted again on evaporation of the liquid. Bromide of cadmium swelled somewhat, but did not dissolve. Iodide of cadmium swelled greatly, and absorbed a large quantity of ammonia, but did not dissolve; it remained enlarged in bulk after evaporation of the liquid. Carbonate of cadmium produced no effect. Sulphide of cadmium was not visibly affected, but it imparted to the liquid a faint cobalt-blue colour.

Fluoride of zinc produced no effect. Chloride of zinc fell to powder, swelled greatly, and did not contract after evaporation of the liquid; it did not dissolve. Bromide of zinc and iodide of zinc behaved like the chloride.

Nitrocyanide of titanium, black oxide of titanium, titanous acid, zirconium scales, oxide of zirconium, sulphate of thorium, glucina, chloride of glucinum, carbonate of glucinum, sulphate of glucinum, sulphate of lanthanum, and niobic acid produced no visible effect.

Oxide of cerium and fluoride of cerium were unaffected. Nitrate of cerium dissolved slightly. Chloride of cerium swelled slightly, but did not dissolve. Metallic magnesium was unaffected. Basic chloride of magnesium dissolved slightly.

Metallic calcium became covered with an insoluble white powder, but did not dissolve nor exhibit any other effect. Fused chloride of calcium fell to powder, swelled greatly, but did not dissolve. Bromide of calcium swelled a little, but did not dissolve. Metallic strontium behaved like calcium. Metallic barium lost its metallic appearance, but showed no signs of solution or other effect. Caustic anhydrous baryta was unaffected. Nitrate of barium dissolved freely, and crystallized on evaporation of the liquid. Chloride of barium swelled considerably, but did not dissolve; it remained enlarged in bulk after evaporation of the liquid. Baric sulphate was not visibly affected.

Metallic lithium produced strong action, and formed a deep indigo-blue coloured fluid: as the liquid evaporated the metal separated, and exhibited successive colours, first that of bronze, then of brass, and ultimately colourless and brilliant like silver. Metallic sodium swelled, and then behaved very much like lithium. Chloride of sodium dissolved sparingly, and did not enlarge in bulk. Metallic potassium behaved like lithium and sodium*. Nitrate of potassium was soluble, but not freely

* These results agree with those previously obtained by C. A. Seely (see 'Chemical News,' vol. xliii. p. 169). Seely found lithium, sodium, potassium, and rubidium soluble, and mercury, copper, thallium, indium, aluminium, and magnesium insoluble in anhydrous liquid ammonia.

so. Fluoride of potassium was very slightly soluble. Chloride of potassium was not visibly affected. Potassic bromide dissolved sparingly, and crystallized on evaporation of the liquid. Iodide of potassium was freely soluble. Silico-fluoride of potassium was insoluble. Chloriridate of potassium crystals were superficially changed to a brown colour, but did not dissolve. Permanganate of potassium dissolved very freely to a deep purple-red solution, and was left behind on evaporation of the liquid. Chromate of potassium dissolved to a minute extent. Potassic bichromate dissolved somewhat, and formed a yellow solution. Chrome-alum was only superficially changed. Fluozirconate of potassium was not visibly affected. Metallic rubidium behaved like sodium and potassium. Fluoride of rubidium was insoluble.

Nitrate of ammonium dissolved very freely. Chloride of ammonium also dissolved very freely, and did not crystallize until nearly all the liquid had evaporated. Carbonate of ammonium, ammonium vanadate, and ammonium metavanadate produced no visible effect.

Uric acid and paracyanogen were unaffected. Argentic cyanide dissolved very freely and rapidly, and crystallized in scales on evaporation of the liquid. Cyanide of mercury dissolved freely, and crystallized only on evaporation of the last portions of the liquid. Cupric cyanide turned deep blue, and dissolved to some extent. Cupric ferrocyanide was converted into a dark green solid. Cyanide of zinc was very little affected. Potassic cyanide was insoluble. Potassic sulphocyanide dissolved to some extent. Ferrocyanide of potassium produced no effect. Fusel-oil, ether, alcohol, aldehyde, and chloroform mixed freely and perfectly. Alloxan became of a deep purple-red colour, and dissolved freely, and left a purple-red solid after evaporation of the liquid. Succinic acid was insoluble. Indigo dissolved sparingly, and formed a reddish-brown solution. Sulphate of quinine dissolved to some extent. Gutta percha imparted a brown colour to the liquid, and a sticky residue was left on evaporation. Paraffin, anthracene, and naphthalin were not visibly affected. Amylene would not mix with the liquid. Mesitylene behaved similarly. Starch swelled and mixed as it does with water, but did not form a clear solution. Glycerine mixed perfectly. White sugar dissolved freely, and left a gummy residue on evaporation of the liquid. Isinglass rapidly became semifluid. Camphor dissolved copiously, and crystallized when the liquid evaporated. Gum-copal dissolved only to a minute extent. Gun-cotton dissolved sparingly, and the solution left a film on evaporation. Bengal silk was quite insoluble.

A few experiments were made for the purpose of examining the chemical properties of the solution of potassium by placing other substances inside the tubes with that metal, and then distilling the ammonia into contact with them. C_2Cl_6 prevented the production of the blue colour. C_2Cl_4 produced chemical action, and prevented the blue colour; the residue was a white solid, exhibiting no signs of free carbon. CBr_4

caused strong chemical action, sparks and flashes of light, the formation of a colourless salt, and prevented the blue colour, and finally produced a violent detonation, by which the whole of the tube was shattered to small particles. Anhydrous carbonate of sodium caused the blue colour first produced to gradually disappear; the residue was quite colourless, and entirely soluble in water. Anhydrous formate of soda produced similar effects. Crystals of oxalate of ammonia produced chemical action, destroyed the blue colour first produced, and left a colourless residue which contained no free carbon. Sesquioxide of chromium did not prevent the blue colour, nor show any signs of reduction or other chemical change. Sesquioxide of uranium prevented the permanency of the blue colour.

I have not specially classified the results of these experiments, but may remark that the only elementary substances soluble in anhydrous ammonia are the alkali metals proper, also iodine (bromine was not tried), sulphur, and phosphorus. The more commonly soluble inorganic salts are nitrates, chlorides, bromides, and iodides; whilst oxides, fluorides, carbonates, sulphides, and sulphates were very generally insoluble. Many saline substances, especially certain chlorides, bromides, iodides, and sulphates, absorbed ammonia copiously, and swelled greatly, but did not dissolve. The behaviour of the chlorides of mercury was peculiar.

In many of these experiments chemical changes took place, and new products were formed; but I did not analyze those products, because that was not my object: these chemical reactions offer a field for future investigators. The sulphides of antimony and cadmium, and the sulphates of mercury and manganese, imparted a purple tint to the liquid; sulphur behaved similarly.

February 6, 1873.

Sir GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

The Right Hon. Hugh Culling Eardley Childers was admitted into the Society.

The following communications were read:—

- I. "On the Osteology of the *Hyopotamidæ*." By Dr. W. KOWALEVSKY. Communicated by Prof. HUXLEY, Sec. R.S. Received December 19, 1872.

(Abstract.)

The paper laid before the Society is intended to fill a certain deficiency in our knowledge of the extinct creation by giving a complete osteology of a family of Paridigitate Ungulata, which, by the completeness of its skeleton, unreduced number of digits, and rich development in generic

and specific forms, I deem to be of great importance in our speculations on the pedigree of living Ungulata Paridigitata.

On theoretical grounds, as well as from the consideration of rudimental parts in living Paridigitata, anatomists have always supposed that fossil representatives of this family, which could be regarded as the progenitors of the recent Paridigitata, would certainly exhibit a much less reduced skeleton and a more complete number of digits than the recent genera do. Yet, strange to say, such complete forms were not forthcoming, and, if assumed on the evidence of their teeth, very little was known about the structure of their bony frame. My statement will sound like an exaggeration; but still it is true, that since the time of Cuvier, who shortly noticed the tetradactyle *Dichobune*, and Blainville, who gave a very imperfect description of *Cainotherium*, we have absolutely not a single paper in which the osteology of an extinct genus of Paridigitata has been fully given*. This may partly be the reason that the pedigree of living genera has hitherto been so obscure.

The Paridigitata of the Paris gypsum, described in a masterly way by Cuvier (the *Anoplotherium* and *Xiphodon*), were clearly extremely reduced descendants of some earlier more complete forms; their feet presented, in fact, nearly the same degree of reduction which we find in our recent Ruminantia, save the confluence in a cannonbone. Seeing the reduced state of their skeleton, how could they be taken as progenitors of the very rich family of Ruminants, some of which have retained, even till our times, a tetradactyle limb? However, so great was the want of some form from which the living Ruminantia could be assumed to be derived, that nearly all comparative anatomists and palæontologists who speculated on these questions of descent, placed the *Anoplotherium* and *Xiphodon* at the head of the series, as the *fons et origo* wherefrom all living Ruminantia have descended.

The present paper is an attempt to introduce to palæontologists a new form, which, though known by its dental system more than twenty-five years ago, has remained totally unknown, so far as its skeleton is concerned. This skeleton, by its completeness, has proved to be a very interesting one, not only in a concrete way, but as furnishing a clue to the understanding of the skeletons of those forms which, though totally unknown, must have preceded *Anoplotherium* and *Xiphodon* in time, and from which these two may have descended.

Besides, the greater importance of the *Hyopotamida* in comparison with *Anoplotherium* and *Xiphodon* lies in the fact, that, while these two last were but poorly differentiated, presenting only two or three distinct specific forms, the *Hyopotamida*, on the contrary, strike us by the extreme diversity and richness of their specific and generic forms. Beginning

* No doubt we have excellent memoirs, like the works of Gaudry, Rutimeyer, Fraas, and H. v. Meyer; but the Paridigitata described in all these do not materially differ from those now living, at least so far as the skeleton is concerned.

in the Middle or Lower Eocene of Mauremont, they existed until the Lower Miocene period; and judging by the great number of species and genera, they must have filled in the fauna of this period the same important place which the greatly diversified Ruminantia fill in the fauna of our own times. Indeed the differentiation of *Hyopotamidæ* may be said to be even greater, in point of size, as they range from the *Hyopotamus Renevieri*, not larger than a rabbit, to the great *Anthracotheerium* of Rochette, which is as big as our Hippopotamus—all the intermediate stages between these two extremes being represented by different genera, subgenera, and species of the same family.

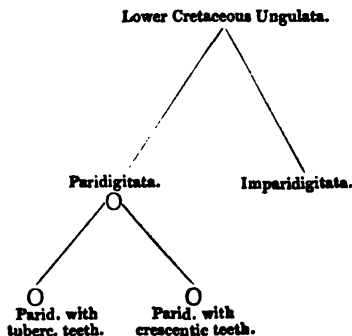
I hope that the rich development of this much neglected family will arouse the attention of palæontologists, and that the skeletons of the different members will be more thoroughly investigated. For my own part, though fully convinced that many of the Eocene *Hyopotamidæ* from Mauremont and Egerkingen present, even in their teeth, characters enough to separate them into distinct genera, I shall not do this, as the multiplication of fossil genera, founded solely on dental characters, without adequate knowledge of the skeleton, is more an obstruction than a help to the progress of palæontology.

This refers to the Eocene *Hyopotamidæ* of Mauremont and Egerkingen; for having found that among the Eocene members of this family there is one which has lost its lateral digits and acquired a didactyle foot, very like an *Anoplotherium*, I was obliged to separate this reduced form from its tetradactyle congeners under the name of *Diplopus* (double foot), while the tetradactyle species of the same family will form the genus *Hyopotamus*. This diversity among the representatives of the same family is very interesting; something of the same kind is, however, to be found in our own times in the *Hyomyschus*, subsisting side by side with the more reduced ruminants, though this is not an entirely parallel case. Moreover, as we have in the *Hyopotamidæ*, so to say, father and son existing together (the complete form together with the reduced), and as, besides, this son bears a great likeness in the typical structure of his limbs to the *Anoplotherium*, we may infer that the fathers of both reduced forms bore also a general likeness; and this gives us a clue to the skeletons of the ancestors of the *Anoplotheridæ*, which is still further strengthened by many other considerations, of which I speak more fully in my paper.

Whilst trying to gain a more complete knowledge of the skeleton of the extinct Paridigitata, I became convinced that we must make some change in our zoological classification of the Ungulata in order to admit the great quantity of genera which have no place in the present system. After the breaking up of the Pachydermata (a name that has long enough obstructed science and really checked progress by holding together the most heterogeneous assemblage of animal forms), all the Paridigitata came to be divided into Suina and Ruminantia. This introduction of a physiological function into a system based on the struc-

ture of the skeleton is objectionable in the highest degree; besides, in this classification there is no room for those fossil genera which are certainly not Suina, and most probably did not ruminate. The greater the number of such genera, the better their organization and history are known, the more pressing the necessity to give them some adequate place in our zoological system. As an instance that such a necessity is keenly felt, we may cite Professor Leidy, who, in describing the *Oreodontidae*, *Agriochoeridae*, &c. of Nebraska, says that they were "ruminating hogs;" but in reality they were not hogs at all, and most probably did not ruminate; what is, then, to be done with them?

The introduction of Professor Owen's* strict division between *Paridigitata* and *Imparidigitata* was a great gain to science; it radically separated two groups that previously were always hopelessly mixed together; but now the same principle must be carried further. The separation of the two groups of *Paridigitata* and *Imparidigitata* took place in very ancient time, not nearer than the Cretaceous period, and the striking diversity exhibited by both groups from the lowest Eocene is a proof of their ancient separation. But one of the branches, the *Paridigitata*, in its turn, split very anciently again into two distinct groups, one with tubercular, the other with crescentic teeth. This occurred at nearest in the Lower Eocene, perhaps even in the Cretaceous period. These groups, once separated, kept entirely apart and followed different lines of descent, although the modifications which both undergo along the descending lines are parallel and analogous even to the greatest details. Following these two divergent lines of descent, both groups culminate in the recent fauna in such forms as the *Phacochoerus* and *Dicotyles* for one group, and the *Bovidae* for the other.



Links between them we discover none; and to discover their parentage, we must pass along the ascending lines to the point at which they diverge, as the linking genera, which doubtless existed at the time of separation, are long ago extinct, and both groups are now widely separated. I suggested this view, whilst studying in the British Museum the remains of *Hyopotamidæ*, to Professor Owen, and he finds no objection to it. He aided me

* Proposed before him by French anatomists, but never carried out completely.

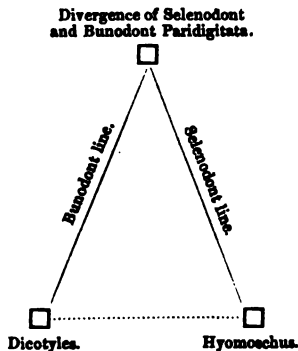
in finding for the two groups convenient names ; and by his suggestion I should call the Paridigitata with crescentic teeth *Par. selenodonta*, and those with tubercular teeth *Par. bunodonta*.

To the *first* group would belong all the ancient and living Paridigitata having crescentic teeth, as the *Anoplotherium*, *Xiphodon*, *Dichobune*, *Anthracotheurium*, *Bothriodon*, *Hypotamius*, *Rhagatherium*, and the living *Ruminantia*.

The *second* would embrace all the Suina, Hippopotamina, and *Entelodon*.

Each one of these two groups may be again subdivided on the principles adopted in this paper.

By such division, we shall gain the advantage of having the Paridigitata arranged into two distinct lines of descent ; every new discovered form will at once have its place along one of the lines, and the true pedigree of both will be ascertained much sooner and with greater accuracy. Whilst now making no such clear division, palæontologists, in projecting their genealogical tables, mix both groups together ; and, according to the need of the moment, they place forms belonging to one line of descent in the other, and *vice versâ*. Thus, for instance, all the Hypotamoids and *Anthracotheurium* are constantly moved about from one line to the other *, while their true place is along the line of Selenodont Paridigitata ; and they have nothing to do with the Bunodont Suina, although groups quite parallel with them may be found on the descending line of Bunodont Paridigitata. Such parallelism, however, does not imply direct links



along parallels drawn across both diverging and descending lines ; the links are to be found only by climbing along the ascending lines to near the point of separation. For instance, *Dicotyles* and *Hyomioschus* occupy analogous positions ; but there is no link between them along the dotted parallel. Links will be found only by going up to the point near their separation.

There are, no doubt, to be found around the points of divergence many forms of which it is difficult to say whether their teeth are tubercular or

* In fact described constantly as Suina. See Gervais, 'Paléontologie de France.'

crescentic, so thick are the lobes ; but once this uncertain stage is passed, both groups keep unmistakably distinct.

Having once become convinced that these two groups of crescent-toothed and tubercular-toothed Paridigitata, after branching off from a common progenitor in the early Eocene (perhaps the Cretaceous) period, followed diverging lines of descent, never mixing together, I tried to ascertain accurately, by such data as were furnished by fossil remains and by lawful induction, what are the exact modifications of the skeleton exhibited by each group along the ascending and descending lines. As these modifications were most clearly given by greater or less reduction of the manus and pes, I subjected these to a detailed comparison.

In tracing the Paridigitata in time, we cannot mistake the tendency clearly manifested by them to a gradual reduction of the manus and pes in such a way that each descendant is always somewhat more reduced than its immediate predecessor. The limbs in the Ungulata serving only for the support of the body, and not for prehension, the organism seems to derive a great advantage from their reduction and simplification.

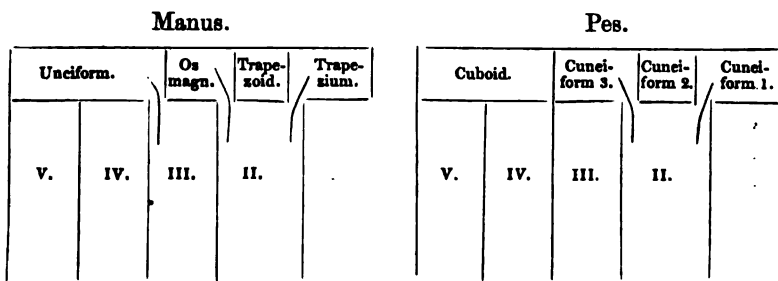
By a comparative study of the least reduced representatives on both lines, I tried to ascertain the probable structure of the manus and pes in the progenitor that has given rise to both groups, or to the whole assemblage of Ungulata ; and this led me to construct a typical manus and pes. On the correctness of this scheme we may to a certain extent rely, as it is exhibited in nearly all its details by the living *Hippopotamus*, the most complete form of the living, and by the *Hyopotamus* and *Anthracotherium*, the most complete of the extinct, Paridigitata. Though such typical foot may be supposed to have been pentadactyle, still, as not a single living or fossil form has ever shown a trace or a rudiment* of the first digit (still less this first digit in a developed state), I thought it more convenient to adhere to facts, and give the foot as it is found in the most complete types, the first digit being always lost, and its carpal and tarsal bone helping to support the second digit. This fundamental typical structure of the manus and pes may be stated, in a few words, to be as follows :—

Supposing the foot to be pentadactyle, the two outer digits (the fourth and fifth) are always supported in the manus and pes by one single bone—the unciform in the manus, the cuboid in the pes ; the three succeeding inner digits are supported each by a separate bone—the third, second, and first cuneiform in the pes, and the os magnum, trapezoideum, and trapezium in the manus. Besides, in the manus, the *third* digit, being supported by the magnum, also touches the unciform by a small ulnar projection, and the *second*, supported by the trapezoides, goes to touch the

* Prof. Huxley noticed this absence of rudiments of the first digit in his Anniversary Address of 1870 (Quart. Journ. Geol. Soc.). Such rudiments of the first digit, described in many cases, have proved always, on examination, to have been mistaken, the trapezium or the first cuneiform being taken as the rudiment of the first digit.

os magnum; the second digit of the pes is supported by the second cuneiform, and by its fibular projection is connected with the third cuneiform. The first digit is lost in all Ungulata, and its typical bone, the trapezium, or first cuneiform, helps to support the second digit.

Diagram of a Typical Foot in Ungulata Paridigitata.



Beginning from this typical structure of the manus and pes, which was probably exhibited by the progenitors of the Paridigitata, we may follow its gradual reduction along both lines of descent in the crescent-toothed (*Selenodont*) and tubercular-toothed (*Bunodont*) Paridigitata. Both lines present a series of parallel modifications, and the parallelism is often carried to the minutest details. The only difference is, that along the crescent-toothed line (*Selenodonta*) the reduction is proceeding at a much quicker rate than along the tubercular-toothed (*Bunodonta*). The reason of this may consist perhaps in the commencing faculty of rumination in the former group, which faculty gave it an immense advantage over the latter. For the comparative anatomist this slow rate of change in the Suina is exceedingly welcome, as it brings the modification of the Suiline foot to our own time, and allows us to discover all the intermediate stages of modification, which, being passed over very rapidly, and in ancient periods, by the crescent-toothed group, have left none or but few traces of their existence.

By the reduction of the foot in Paridigitata, I simply mean that the function of locomotion which has been performed primitively by all the four (or five) digits begins to be carried on chiefly by the middle two, the lateral digits undergoing a gradual decrease. This, as I have said before, seems to be of great advantage to the organism, and is manifested by all descending lines of Ungulata.

In trying to ascertain the exact method of this reduction and its final results in recent and fossil genera, we come to very interesting facts that have not been duly noticed before, and which furnish us with the explanation of the presence of so many very reduced forms even in old Eocene and Miocene deposits. In both groups, the crescent-toothed (*Selenodonta*) and the tubercular-toothed Paridigitata (*Bunodonta*), we meet with a two-

fold mode of reduction of the manus and pes,—a simple or *inadaptive*, and an elaborate or *adaptive* mode.

Following the first or *inadaptive* mode of reduction, the foot, whilst losing its lateral digits, acquires no better adaptation to altered conditions of locomotion and support of the body than that which is derived from the mere thickening of the remaining digits. The relation between the carpal and tarsal bones and the remaining two middle metacarpals and metatarsals remains just the same as it was in the tetradactyle ancestor. The remaining digits do not exhibit any modification by which they receive more ample support from the carpal and tarsal bones, by taking the place formerly occupied by the now reduced and lost lateral digits. This mode of reduction I call *inadaptive*, or reduction in which *inheritance is stronger than modification*. As an instance of this *inadaptive* mode of reduction, I may point out the foot of *Anoplotherium* and *Xiphodon*. The annexed diagram clearly illustrates



this mode of reduction. The fourth digit does not even take the whole of the unciform, and a part of this bone is still occupied by the useless rudiment of the fifth digit; the third has not extended over the whole os magnum; and the useless rudiment of the second digit occupies its typical place on the trapezoid, touching the os magnum, and being additionally supported by the trapezium.

Following the second or *adaptive* mode of reduction, the middle digits grow larger and thicker than in the first mode; but whilst broadening transversally they do not adhere to the ancestral pattern, but tend to gain a better support on *all* the bones of the carpus and tarsus; they deviate from the ancestral type, push the lateral digits (while these are yet completely developed) to the side, and usurp their typical carpal and tarsal bones for their (the middle digits) own use, thus gaining a better and more complete support for the body. The lateral digits, deprived of their typical carpal and tarsal bones, and taking henceforth no active part in locomotion, tend to disappear; and every millimetre that is lost by the lateral digits is immediately taken possession of by the enlarged middle ones; so that even before the entire disappearance of the lateral digits the two middle digits have usurped the whole of the distal surface of the carpus and tarsus, the fourth digit has spread over the whole unciform (manus) and cuboid (pes), and the third has taken possession of the trapezoid (manus) and second cuneiform (pes). This once attained, the two middle digits, being pressed from both sides by the carpal and tarsal bones, begin to coalesce, forming the so-called *cannon* of the recent Ruminantia, or of the hind foot of *Dicotyles*. This mode of reduction I call the *adaptive*, or reduction in which such *modification keeps pace with inheritance*.

As an instance of this mode, I may cite the foot of *Sus*, *Dicotyles*, *Hyomochus*, Ruminantia. Every anatomist will acknowledge that this second mode of reduction is much more useful to the organism than the first.

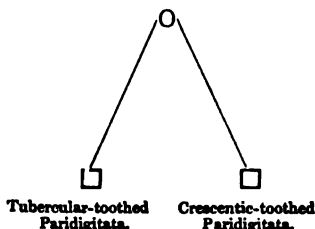
If we inquire further what are the genera which follow the first or *inadaptive* mode of reduction, we find that *all extinct* genera of Paridigitata follow it, while all *living* * genera follow the second or adaptive mode of reduction.

This being the state of the case, the questions arise, Did they not become *extinct* because of their incapacity to adapt themselves completely to altered circumstances? and did not the others *survive* because they adapted themselves more fully to these circumstances? I will try to consider both cases in reference to the living and fossil Paridigitata.

I said before that early in the Eocene period the group of Paridigitata split dichotomously into two secondary groups, one with crescentic teeth, the other with tubercular; the first I have called the Selenodonta, the second Bunodontia (or Suina). Now each of these secondary groups fol-

* Or fossil forms which continue to live, or have left direct successors, as *Palæotherium* and the Miocene Ruminantia from Auvergne.

Early Eocene Paridigitata.



lowed a twofold mode of descent, one of which I term the *inadaptive*, and the other the *adaptive*, thus, finally, giving rise to four distinct groups:—

Paridigitata with
crescentic teeth
(Selenodonta).

A. The group following the *inadaptive* reductions develop enormously in Eocene and Middle Miocene times: all have distinct metacarpalia and metatarsalia, five-lobed upper molars, smooth distal extremities of the metapodials. Genera: *Bothriodon*, *Dichobune*, *Rhagatherium*, *Cainotherium*. They reached their highest development and culminated in the didactyle *Anoplotherium*, *Xiphodon*, and *Diplopus*, which all became extinct without direct successors.

B. The group following the *adaptive* reduction separated from the group A somewhere in the Middle Eocene, by some of the small *Hyopotamidae* acquiring four-lobed upper molars, as met with at Mauremont, and becoming *Dichodonts*. Intermediate stages little known; the *Gelacus* is one of them. The least reduced living form is *Hyomoschus*. Culminating in recent times in the didactyle *Bovidae* and *Antilopidae*.

Paridigitata with
tubercular teeth
(Bunodonta or
Suina).

A. Group following the *inadaptive* reduction very little known. *Acotherulum* and another larger hog-like animal from the Middle and Upper Eocene may belong to this group; they were certainly tetradactyle. Culminated in the lowest Miocene in the didactyle *Entelodon*: no successors.

B. Group following the *adaptive* reduction, branched from the group A in the Eocene; the most typical representative is the *Chaerotherium* from Sansans, with the phalangeal ridge not yet extending over the whole distal end of the metapodium. *Palaeochaerus*: reduction has fairly set in on the adaptive mode, the phalangeal ridge passing over the whole end of metapodial. *Sus* still more reduced. *Dicotyles*: all the distal surface of the carpus and tarsus taken by the enlarged middle digits. Tending to become *didactyle*.

We must briefly consider each of these groups.

The Paridigitata with crescentic teeth following the *inadaptive* mode of reduction, and whose skeletons are known, are the *Anoplotherium*, *Xiphodon*, *Anthracotheridæ*, and *Hyopotamidæ*. If it should be asked *why* they followed this mode of reduction, the reason is obvious. Admitting that an advantage is gained by the simplification of the foot and the reduction of the number of digits, this mode of reduction is the most simple course to be taken. We must imagine the enlargement of the middle digits to be accompanied by a broadening of their correspondent bones in the carpus and tarsus; the trapezoideum and the second cuneiform were simply pushed aside (not made use of) by the enlargement of the third digit, and their reduction kept pace with the reduction of the second digit. If we think how the process must have gone on "*in naturâ*," we shall find that it required quite an unusual occurrence, some happy chance, for the third digit to go over the separating line between the magnum and trapezoideum, or the third and second cuneiform, and get a footing on these last bones, which typically belonged to the second digits. This was evidently the most advantageous mode, but it did not occur at once; and the organism has taken the more simple and obvious *inadaptive* mode, which, once fairly set in, could not be changed. This branch of the Paridigitata then, starting from their tetra- (or penta-)dactyle progenitors in the Cretaceous or earliest Eocene, arrived at the close of the Eocene (from which strata alone we have Paridigitata whose skeletons are known) to the reduced didactyle forms, known as the *Anoplotherium* and *Xiphodon*. That these last had tetradactyle ancestors is supposed, on theoretical grounds, by the evolutionists; besides, their rudimental second and fifth digits point clearly to some form in which these rudiments were completely developed and used for locomotion.

Whilst trying to ascertain the structure of the skeleton of an extinct family (*Hyopotamidæ*) allied to the *Anoplotheridæ*, but which was supposed to be chiefly Miocene, I found that the Miocene genera could be regarded only as the last representatives of this exceedingly numerous family, whose chief development fell in the Eocene times, when it was represented by numerous subgeneric and even generic forms. I was fortunate enough to find, in the collection of M. Aymard, at Puy, a large assemblage of bones belonging to the oldest Miocene representative of this family, the *Hyopotamus*; indeed so much, that I could completely restore the limbs and nearly the whole skeleton. The limbs prove to be tetradactyle, with well-developed lateral digits. The same family is so richly developed in the Eocene, that we have a full right to suppose that the older genera had even a more completely developed manus and pes.

From Puy I came to London to complete my study, as teeth which were not to be distinguished from the *Hyopotamus* of Puy were known to be numerous in England; and whilst studying the bones found in England, I was struck by the fact that some of these belonged to a didactyle genus

of the same family, which in England proved to be associated with the tetradactyle genus. To this new genus of the Hyopotamoid family I gave the name *Diplopus*. This was indeed a welcome discovery—ancestor and descendant existing together, the complete with the reduced form living about the same period*. Moreover, the didactyle form bore a great general likeness in the structure of the limbs to *Anoplotherium* and *Xiphodon*, being perhaps only a little more elaborate and better adapted than these first experiments of the Eocene times.

The likeness of the descendants allowed me to make inferences as to the likeness of the ancestors; and, taking into consideration the structure of the limbs in the tetradactyle *Hyopotamus*, and the rudimental second and fifth digit still existing in *Anoplotherium*, *Xiphodon*, and *Hyopotamus*, I feel confident that the supposed ancestor of the first two did really possess a *manus* and *pes* very like the projected typical diagram; indeed we may be nearly as confident of this as if we had found the actual thing imbedded complete in some early Eocene or even Cretaceous rock.

This, then, was the state of things in the earliest Eocene; large numbers of Paridigitata with tetradactyle feet like our *Hyopotamus*, and the supposed progenitors of *Anoplotherium* and *Xiphodon*, represented the group of Paridigitates with crescentic teeth (*Selenodonta*). Reduction in the number of digits, being an advantage to the organism, was steadily going on. But, be it observed, we follow now the *inadaptive* line of descent; and while the whole weight of the body was, by gradual steps, entirely transferred to the two middle digits, these thickened and grew larger, but entered into no special adaptation by means of which they should better perform the work which had fallen to their share; they did not enlarge so as to gain additional support from all bones of the second row of the carpus and tarsus; the reduction was *inadaptive*: inheritance is in them stronger than modification.

Seeing that old Paridigitata present only two free metacarpals and metatarsals, and that recent Ruminantia have the same two metacarpals and metatarsals coalesced into a single cannonbone, evolutionists generally rush at the seemingly obvious conclusion that once the tetradactyle foot reached the reduced state of two digits, these coalesced together, and were transformed into the cannonbone of Ruminants. No such thing, however, happened; nor could it have happened with the old didactyle Paridigitata, as the *Anoplotherium*, *Xiphodon*, and *Diplopus*; and the reason why it could not is clearly indicated by the structure of their feet. We have already shown that, following this *inadaptive reduction*, the two middle digits, whilst growing larger, continue to occupy only the inner half or more of the unciform and the greater part of the os magnum; so that from the outer as well as from the inner side the carpal bones which support useless rudiments overhang the two middle functional digits. In con-

* Such cases are numerous. In the Sewalik Hills the *Hipparion* is associated with the horse.

sequence of this, the distal surface of the carpus was much broader than the proximal surface of the two functional digits—an arrangement not calculated for firm equilibrium. Now the confluence of the two middle digits is always followed by a considerable contraction; and if this coalescence should occur in the imperfectly adapted foot of *Anoplotherium*, and especially *Xiphodon*, all equilibrium would be lost. If ever such confluence occurred, by reason of the tendency to the greatest possible reduction, the resulting form had not the least chance of being propagated and of holding its ground against the competing genera. The broadening of the middle digits could not occur after the entire loss of the laterals; and we shall see that, in genera which have left immediate successors (*Sus*, *Hyomoschus*), the lateral digits are not allowed to go until the middle ones have obtained a secure footing on the entire distal surface of the carpus and tarsus. However, these inadeptively reduced genera of the Eocene could perhaps have lived till our own days; but the development of the competing and better adapted forms pressing them on all sides, they had no chance to stand their ground against them, and became extinct without any direct posterity, while the succession of the Paridigitata Selenodonta was carried by a side branch, and reached its culminating point in the Miocene, continuing from thence to our own days.

We turn now to the same mode of *inadaptive* reduction as manifested by the tubercular-toothed Paridigitata (*Bunodonta*), or Suina. The old representatives of this group are very little known. The *Chceropotamus* is a very doubtful genus, and may be inclining towards the crescentic-toothed Paridigitata, being supposed to be the progenitor of the *Anthracotheridæ* and *Hyopotamidæ*. Besides it we have the *Acotherulum saturninum*, Ger., a truly tubercular-toothed Paridigitate from the Upper Eocene, *Acotherulum Campichii* (*Dichobune*, Camp. Pictet) from the Lower Eocene of Mauremont, and a larger pig-like animal from the same deposit not yet described or named. These are undoubtedly the oldest tubercular-toothed Paridigitates we know; but unfortunately our knowledge is based only on dental characters. However, considering that even the recent Suina have not yet completely lost their two lateral digits, it may, with the greatest probability, be inferred that these old Eocene forms were tetradactyle. Our knowledge of the development of this group is very incomplete; but there can be no doubt that, though not nearly so rich as the Selenodont group, they were still numerous, as may be inferred from the great quantity of the Suina in the Miocene, and such forms as the *Listriodon splendens* *. We are so accustomed to look on the Suina as a group of tubercular-toothed *tetradactyle* Paridigitata, that no one ever thought of the possibility of a *didactyle* hog; but, strange as it may seem, such a Suiline animal

* I have not been so fortunate as to see any bones of the *Listriodon*; but as this miocene hog died without any successors, I should not be astonished if it proves to be *didactyle*, thus being a parallel to *Hyopotamus* in the same sense as *Entelodon* is parallel to *Anoplotherium*.

existed ; stranger still, it existed in such an ancient period as the close of the Eocene in the lowest strata of Ronzon at Puy. This is the *Entelodon*, Aym. (*Elotherium*, Pom., *Archæotherium*, Leidy). The Suilline characters are so striking in this form, that it was at once placed among the Suina, and pronounced tetradactyle, though the confluent tibia and fibula (mentioned by Leidy) might have been taken as a warning against rash conclusions. I have found in the cabinet of M. Aymard, in Puy, some bones of this animal ; few it must be acknowledged, but still leaving no doubt as to the didactyly of *Entelodon*. Of this I shall try to adduce more extensive proofs in a forthcoming memoir on this genus. How can the presence of a hog with such reduced limbs be explained in such ancient deposits, when even the living *Suidæ* have not yet reached this stage of reduction ? The fact, however, is intelligible when we consider that the *Entelodon* is the final result of the *inadaptive development* and reduction along the line of tubercular-toothed Paridigitata ; it is the *culmination point* of this group, and in this sense quite parallel to the *Anoplotherium* in the other group. Thus the Paridigitata, which split dichotomously in the earliest Eocene (?) into two groups, the tubercular-toothed (*Bunodontia*) and the crescent-toothed (*Selenodontia*), following the *inadaptive* mode of reduction, reached their *culmination point* in the Upper Eocene or just above it, in such forms as *Entelodon* for the first group, and *Anoplotherium*, *Xiphodon*, *Hyopotamus* for the second group, which all became extinct without any direct posterity. The living Suina and Ruminantia are not directly connected with them, but are the issue of lateral branches which followed the *adaptive* mode of development and reduction.

We may now consider the results of the *adaptive* mode of reduction. As I said before, the rate of this reduction is much slower in the tubercular-toothed Paridigitata, or Suina ; and this gives us the means of following more closely all the stages of reduction. I propose, therefore, in the first place, to consider these.

Though the published materials, as far as the skeleton is concerned, are very poor, we have the means of giving nearly all the intermediate stages between those genera in which the manus and pes are conformable to the true tetradactyle type, every digit (except the fourth and fifth, which are always borne by one) being carried by a separate carpal and tarsal bone, and those in which the entire distal surface of the carpus or tarsus is taken by the enlarged two middle digits.

The adaptation of these two middle digits on the *adaptive* line forms a striking contrast to their rigidity exhibited by the other mode of reduction ; and we shall briefly indicate the stages by which the typical Suilline foot actually passed to reach the stage exhibited now by *Dicotyles*.

We are at a total loss to indicate the precise time when the *adaptive* branch separated from the *inadaptive* ; it was certainly somewhere in the lowest Miocene, as in the Middle Miocene we find already a large quantity of Suinæ in which the *adaptative* reduction has fairly set in. As the first

stage I must consider a small Suilline animal, though not the oldest, but perhaps a remnant of the older type; this is the *Chærotherium*, Lart., from Sansans. The primitiveness of this small pig is indicated by the fact that the carpal and tarsal bones retain their typical relation to the four metacarpals and metatarsals; the humerus is very *Anoplotherium*-like, and the distal extremity of the metapodium is smooth anteriorly, the phalangeal articular ridge being limited only to the palmar side, as in all ancient Paridigitata.

First Stage, *Chærotherium*, Lart., Sansans.—The middle digits are enlarged, but the laterals still retain their typical relation to the supporting bones of the carpus and tarsus*. Distal end of humerus *Anoplotherium*-like (ancient); the proximal end of the radius, in correspondence with the humerus, is also *Anoplotherium*-like. The distal end of metapodium is smooth, the phalangeal ridge being limited to the palmar side.

Second Stage, *Palæochærus* (Allier).—The adaptive reduction of the *manus* and *pes* has fairly set in, its first indication being that the radial margin of the third digit (in the *manus* and *pes*) is raised in such a way as to exclude the second digit from going to its typical facet on the os magnum and third cuneiform, though leaving it still in the full possession of the trapezoid and second cuneiform. The phalangeal articular ridge is passing from the palmar side round the distal extremity to the anterior face of the metapodium.

Third Stage, *Suidæ*.—Adaptive reduction is proceeding further, the middle digits are greatly enlarged, and the third digits of the *manus* and *pes* spread over one half the trapezoideum and nearly the whole of the second cuneiform. The lateral digits touch the ground only very slightly, and are not important for locomotion.

Fourth Stage, *Dicotyles*.—The middle digits are so enlarged and adapted that the entire distal surface of the carpus and tarsus is taken by them; the lateral digits have no distinct facets on the distal surface of the carpus, and are merely hanging to the enlarged middle digits. The fifth digit of the *pes* is lost, and the two metatarsals are coalesced into a cannonbone; the metacarpals are also so closely pressed together that their confluence is imminent. The complication of the stomach, which is divided into three chambers, shows a beginning of rumination, slight traces of which are even exhibited by the common hog; the premolars become complicated, and begin to assume the shape of molars†, the first premolar is lost (as in all Ruminants), the incisors reduced to four, the canines are small.

Fifth Stage.—The culminating point is not yet reached by the tubercular-

* That is, the second digit is supported by the trapezoideum, and has besides a facet on the os magnum, as in *Hippopotamus*, or in the typical tetradactyle foot generally.

† A very important circumstance, considering that we meet the same fact in other groups where the premolars assume the shape of molars, as in *Palæotheriæ*, horses, rhinoceros, &c.

toothed Paridigitata, following the adaptive mode of reduction; but as it was reached by the same group on the *inadaptive* mode (*Entelodon*), and as the parallel group of crescent-toothed Paridigitata, whose reduction is going at a quicker rate, has already reached it, there can be no doubt that the Suina are tending also to the same culminating point. In reaching it the lateral digits will be entirely lost, the trapezium will coalesce with the magnum, and the second cuneiform with the third; the middle metacarpals and metatarsals will coalesce into a complete cannonbone, and probably the stomach will become still more complicated, and they will ruminate. That this state is the goal towards which the Suina tend I have little doubt; but it is more than probable that man by his influence will prevent them from ever reaching it.

Our task is more difficult when we come to inquire into the line of descent which has given rise to the Ruminantia. As stated before, I cannot put the *Anoplotherium*, nor the *Xiphodon*, in their pedigree. In my opinion, the line which ends in Ruminantia branched off from the small tetradactyle *Hyopotamida*, which were so numerous in the Eocene period. I find in the Eocene of Mauremont all stages of transition between the *five-lobed upper molars* of these *Hyopotamida* and teeth having a true ruminant four-lobed pattern; these last have belonged to some small species of *Dichodon*. Unfortunately we have no clue to the skeleton, though, seeing the tetradactyle living *Hyomachus*, it may fairly be assumed that these early progenitors of Ruminantia were also tetradactyle. The small tetradactyle *Cainotherium* is a very tempting genus in speculations about the descent of Ruminantia; but I must exclude it for many reasons, though I cannot here give them in full. Some of these are as follows:—the *Cainotherium* retained till the Middle Miocene five-lobed teeth on the *Dichobune* pattern (with the three lobes on the posterior half of the tooth), while we have truly ruminant teeth already in the Eocene; it retained its upper incisors and free metatarsals, while the much older *Gelacus*, Aym., which is already a true ruminant, had no upper incisors and the metapodials were confluent in the adult. *Cainotherium* seems to be a direct descendant of *Dichobune*, and to have become extinct, without leaving any successors.

Supposing that the *Dichodon* had a foot true to the tetradactyle type, we do not find the earliest stages of reduction; they were passed rapidly, and in very ancient times: but there can be little doubt that the Ruminantia began with a tetradactyle foot, and ended by a cannonbone adapted to the whole distal surface of the carpus and tarsus. Such adaptation of the two middle digits could not be obtained at one leap; and certainly all stages between a tetradactyle foot (in which every digit was supported by a separate bone in the carpus and tarsus) and a didactyle foot (in which the two enlarged middle digits have taken the whole distal surface of all the carpal and tarsal bones) were passed by this group in the same manner we have seen it in the Suina, but only a

few traces of this passage remain. From the tetradactyle *Dichodon*, the group of adaptive Selenodonts may be said to have split in two subordinate groups. In one of these, represented by the *Hyomoschus*, the lateral digits are retained, and only the metatarsals become confluent, while the two middle metacarpals continue to be free*. In the *Tragulidæ* the two middle digits coalesce, in both fore and hind limbs, into a complete cannonbone, but the lateral digits are still retained in their whole length as useless, nearly filiform appendages. The distal surface of the metapodium remains smooth; the rumination is incomplete.

In the other group, as the representative of which we may cite the *Gelocus*, Aym., the lateral digits were soon lost, and the remaining two middle digits have taken the entire distal surface of the carpus and tarsus; still they remain separate, perhaps through life, in some of the Eocene *Geloci* whose remains I have seen from the phosphatic limestone deposits in the south of France, near Cahors, in a locality called Caylux. In this deposit the bones of *Gelocus* are found, together with large *Anoplotheria* and *Palaotheria*, and even the completely ossified and not epiphysed metatarsals are found entirely free. In the lowest Miocene of Puy, however, we find a *Gelocus* whose metacarpals and metatarsals are free only in the young, and coalesce in the adult; but, even after their coalescence, the distal end of the metapodium is smooth, and the articular ridge is limited to the palmar side. In the somewhat newer (about the upper part of the Lower Miocene) deposits of Allier, in Auvergne, we meet at last with metatarsals and metacarpals entirely coalesced into a complete cannonbone, and the articular ridge taking the whole distal extremity of the metapodium. Small rudiments of the lateral digits (second and fifth) still remain as styliform appendages on both sides of the cannonbone, in the fore and hind limbs.

Such true ruminant forms are exceedingly numerous in the Miocene of Allier; they are all hornless, and some retain seven molars in the lower jaw, as in all ancient Selenodonts. In most, however, of these newer Miocene forms the first premolar of the lower jaw is lost, and they exhibit the same dental formula as the living Ruminantia, from which they seem not to differ in any of the essential characters. These true ruminant forms of the Lower Miocene may be considered to have reached the culmination point of their reduction, and we shall consider them as such. Thus the Selenodont *Paridigitata*, after branching off from the common stock in the Lower Eocene, reach the utmost stage of reduction on the adaptive mode a little below the Middle Miocene; this we consider to be the fifth stage, or the culmination.

The fifth stage, or the culmination point of the *Paridigitata* Selenodonts,

* These middle metacarpals and metatarsals are enlarged and adapted to the whole distal surface of the carpus and tarsus.

following the adaptive mode of reduction, means that the reduction of the manus and pes was carried so far that it could not proceed further; this point was attained already in the Lower Miocene. When once the metapodium was reduced to *one* bone, and this one had taken the whole distal surface of the carpus and tarsus, any further reduction or improvement was quite impossible. Besides, the completely developed faculty of rumination gave these forms an enormous advantage over the other, non-ruminant, Paridigitata occurring in the same strata. They could live on such matters as twigs, bark of trees, mosses, lichens, on which no other Ungulata can subsist; such food is found everywhere, requires no cunning and very little struggle to get it. All essential modifications were attained very early, and the chief of these are the confluence of the two middle digits in a complete cannonbone and rumination. Then began the luxury of all sorts of appendages—excrecences on the frontal bones covered with skin, uncovered by skin in the form of prickly simple horns (*Pudu*), or double (*Dicroceras* of Sansan, Muntjac), then branched and palmated. In other groups these bony cores were covered with horny sheaths, which at first differed but little from agglutinated hairs (*Antilocapra americana*), then became more compact, as in the smooth and hard horny sheath of the hollow-horned Ruminantia. These secondary characters were all acquired, thanks to abundant time, after the essential characters of the type had been assumed; if man had come on earth a little later than he did, he certainly would have found nearly parallel cases in the group of Suina, monodactyle (with cannonbone) hogs with different appendages. As it is, he stopped the course of events; all further improvement is out of the question, or only possible in such groups as the Rodentia, who prey on man's food, being at the same time independent of him.

It may be asked, How stands the matter in the Imparidigitate Ungulata? And though I cannot enter fully into the case, I may state that the same course of events is observable in them; only there could be no *in-adaptive* reduction, as the body could not, under any circumstances, be held in equilibrium upon one single third digit, if this one had not taken the whole distal surface of the carpus and tarsus. But the task in this group was much more difficult; to get one middle digit to perform the work shared in the ancestors by five, and in the immediate progenitor by three, required time. To accomplish this, two geological periods were needed; but still, by the incessant tendency to reduction, the work was done, and the monodactyle horse spread over the surface of the globe, superseding all other Imparidigitata, which are evidently rapidly dying out. The only *two* genera which remain still, the Rhinoceros and the Tapir, cannot last long. But this spreading and multiplication of the *Equidæ* was also accompanied by a total change of diet: from an omnivorous animal it became grass-eater; and indeed, by its teeth and many other characters, the horse is very *analogous* to the Ruminantia,

being, as they are, the culmination point of the group of *Imparidigitata*. The reduction of the horse foot, however, is not fully accomplished yet; to attain this, the styliform metatarsals and metacarpals (the second and fourth) have to be lost.

- II. "Magnetic Survey of Belgium in 1871." By the Rev. S. J. PERRY. Communicated by Sir EDWARD SABINE, K.C.B., F.R.S.
Received January 24, 1873.

(Abstract.)

The magnetic observations which furnished the results contained in this paper were made during the autumn months of 1871.

The instruments used and the methods adopted were almost identical with those employed in previous magnetic surveys of France.

The dip was observed by Mr. W. Carlisle, magnetic assistant of Stonyhurst Observatory, and the rest of the observations were taken by the Rev. S. J. Perry.

This new series of determinations of the terrestrial magnetic elements was rendered the more necessary, as preceding observers had chosen very few stations in Belgium, and as the curvature of the isodynamics and isoclinals in Dr. Lamont's maps of Belgium, Holland, and North-west Germany indicated a very considerable disturbing cause in the first-named country.

The values obtained in 1871 are a strong confirmation of the suspicions of irregularity to which former observations had given rise; for although the lines of equal dip, declination, and horizontal force bear a sufficiently close resemblance to those of neighbouring countries, there is evidence of much disturbance; and when the values of the dip and horizontal force are combined, the isodynamics show clearly that the coal-measures, which stretch completely across the south-east portion of Belgium, exercise a strong disturbing influence. This local magnetism might be incapable of producing more than a decided curvature of the isodynamics of an extended tract of country; but when all the stations of observation are situated within narrow limits, the perturbation completely masks the normal direction of the lines.

The following is a complete list of the magnetic elements observed at the different stations, and reduced to the common epoch of January 1, 1872:—

Station.	Declination.	Dip.	Horizontal force.	Intensity.
Aix-la-Chapelle	16°464	66°637	4·0064	10·1025
Alost	17°349	67°210	3·9518	10·2016
Antwerp	17°489	66°999	3·9296	10·0559
Arlon	16°398	65°907	4·1175	10·0857
Bruges	17°938	67°155	3·8950	10·0321

Station.	Declination.	Dip.	Horizontal force.	Intensity.
Brussels.....	17°59	66°975	3·9613	10·1271
Courtray	17°756	66°678	4·0028	10·1103
Ghent	17°823	67°221	3·9197	10·1232
Liège	16°233	66°464	4·0145	10·0522
Lierre.....	66°948		
Louvain	16°824	66°898	3·9565	10·082
Mechlin	66°714		
Mons	17°216	66°573	4·0065	10·0767
Namur	17°541	66°538	3·9941	10·0311
Ostend	18°097	67°211	3·9152	10·1077
Spa.....	16°627	66°653	4·0239	10·1531
Tournay.....	17°691	66°632	3·9975	10·0776
Tronchiennes.....	17°867	67°361	3·9032	10·1397
Turnhout	17°025	67°113	3·9542	10·1665
Verviers.....	66°718		
Secular variation ..	-0·1255	-0·0573	+ 0·00542	-0·01155

February 13, 1873.

Rear-Admiral RICHARDS, C.B., Vice-President, in the Chair.

The following communications were read :—

- I. "On Curvature and Orthogonal Surfaces." By A. CAYLEY, F.R.S., Sadlerian Professor of Mathematics in the University of Cambridge. Received December 27, 1872.

(Abstract.)

The principal object of the present Memoir is the establishment of the partial differential equation of the third order, satisfied by the parameter of a family of surfaces belonging to a triple orthogonal system. It was first remarked by Bouquet that a given family of surfaces does not in general belong to an orthogonal system, but that (in order to its doing so) a condition must be satisfied: it was afterwards shown by Serret that the condition is that the parameter considered as a function of the coordinates must satisfy a partial differential equation of the third order; this equation was not obtained by him or the other French geometers engaged on the subject, although methods of obtaining it, essentially equivalent but differing in form, were given by Darboux and Levy; the last-named writer even found a particular form of the equation, viz. what the general equation becomes on writing therein $X=0$, $Y=0$ (X , Y , Z the first derived functions, or quantities proportional to the cosine-incli-

nations of the normal). Using Levy's method, I obtained the general equation, and communicated it to the French Academy. My result was, however, of a very complicated form, owing, as I afterwards discovered, to its being encumbered with the extraneous factor $X^2 + Y^2 + Z^2$: I succeeded, by some difficult reductions, in getting rid of this factor, and so obtaining the equation in the form given in the present memoir, viz.

$$\begin{aligned} & ((A), (B), (C), (F), (G), (H)) \chi \delta a, \delta b, \delta c, 2\delta f, 2\delta g, 2\delta h) \\ & - 2((A), (B), (C), (F), (G), (H)) \chi \bar{a}, \bar{b}, \bar{c}, 2\bar{f}, 2\bar{g}, 2\bar{h}) = 0 : \end{aligned}$$

but the method was an inconvenient one, and I was led to reconsider the question. The present investigation, although the analytical transformations are very long, is in theory extremely simple: I consider a given surface, and at each point thereof take along the normal an infinitesimal length ρ (not a constant, but an arbitrary function of the coordinates), the extremities of these distances forming a new surface, say the vicinal surface; and the points on the same normal are considered as corresponding points, say this is the conormal correspondence of vicinal surfaces. In order that the two surfaces may belong to an orthogonal system, it is necessary and sufficient that at each point of the given surface the principal tangents (tangents to the curves of curvature) shall correspond to the principal tangents at the corresponding point of the vicinal surface; and the condition for this is that ρ shall satisfy a partial differential equation of the second order,

$$((A), (B), (C), (F), (G), (H)) \chi (d_x, d_y, d_z)^2 \rho = 0,$$

where the coefficients depend on the first and second differential coefficients of U , if $U=0$ is the equation of the given surface. Now, considering the given surface as belonging to a family, or writing its equation in the form $r - r(x, y, z) = 0$ (the last r a functional symbol), the condition in order that the vicinal surface shall belong to this family, or say that it shall coincide with the surface $r + \delta r - r(x, y, z) = 0$, is $\rho = \frac{\delta r}{V}$,

where $V = \sqrt{X^2 + Y^2 + Z^2}$, and X, Y, Z are the first differential coefficients of $r(x, y, z)$, that is, of the parameter r considered as a function of the coordinates; we have thus the equation

$$((A), (B), (C), (F), (G), (H)) \chi (d_x, d_y, d_z)^2 \frac{1}{V} = 0,$$

viz. the coefficients being functions of the first and second differential coefficients of r , and V being a function of the first differential coefficients of r , this is in fact a relation involving the first, second, and third differential coefficients of r , or it is the partial differential equation to be satisfied by the parameter r considered as a function of the coordinates. After all reductions, this equation assumes the form previously mentioned.

II. "On a new Relation between Heat and Electricity."

By FREDERICK GUTHRIE. Received January 10, 1873.

(Abstract.)

It is found that the reaction between an electrified body and a neighbouring neutral one, whereby the electricity in the neutral body is inductively decomposed and attraction produced, undergoes a modification when the neutral body is considerably heated.

Under many circumstances it is found that the electrified body is rapidly and completely discharged. The action of discharge is shown to depend mainly upon the following conditions:—(1) the temperature of the discharging body and its distance from the electrified one; (2) the nature (+ or —) of the latter's electricity.

With regard to (1), it is shown that the discharging power of a hot body diminishes as its distance increases, and increases with its temperature; but, concerning the temperature, it is proved that the discharging power of a hot body does not depend upon the quantity of heat radiated from it to the electrified body, but chiefly upon its quality. Thus a white-hot platinum wire connected with the earth may exercise an indefinitely greater discharging power, at the same distance, than a large mass of iron at 100° C., though the latter may impart more heat to the electrified body.

Neither the mere reception of heat, however intense, by the electrified body, unless the latter have such small capacity as to be itself intensely heated, discharges the electricity if the source of heat be distant; nor is discharge effected when the electrified body and a neighbouring cold one are surrounded by air through which intense heat is passing. But, for the discharge, it is necessary that heat of intensity pass to the electrified body from a neutral body, within inductive range.

White- and red-hot metallic neutral bodies exercise this discharging power even when isolated from the earth, but always with less facility than when earth-connected.

The hotter the discharging body, whether isolated or earth-connected, the more nearly alike do + or — electricities behave in being discharged; but at certain temperatures distinct differences are noticed. The — electricity, in all cases of difference, is discharged with greater facility than the +.

Attempts are made to measure the critical temperatures at which earth-connected hot iron (1) discharges + and — electricity with nearly the same facility, (2) begins, as it cools, to show a preferential power of discharging —, and (3) ceases to discharge —. The temperatures so obtained are measured by the number of heat-units, measured from 0° C., in 1 gram of iron of the respective temperature, represented by the value of the expression $F\theta \Sigma u$.

It is shown that various flames, both earth-connected and isolated, have an exceedingly great power of discharging both kinds of electricity.

The effects in regard to discharge are shown to be similar when platinum wire, rendered hot by a galvanic current, is used, and also when the condensed electricity of a Leyden jar is experimented on.

As hot iron shows a preferential power of discharging — over + electricity, so it is found that white-hot but isolated iron refuses to be charged either with + or — electricity. As the iron cools, it acquires first the power of receiving — and afterwards of receiving +. Further, while white-hot iron in contact with an electrified body prevents that body from retaining a charge of either kind of electricity, as it cools it permits a + charge to be received, and subsequently a — one.

A suggestion is made as to the existence of an electrical coercitive force, the presence of which together with its diminution by heat would explain much of what has been described.

February 20, 1873.

Rear-Admiral RICHARDS, C.B., Vice-President, in the Chair.

The following communications were read :—

- I. "On the Anatomy and Histology of the Land-Planarians of Ceylon, with some Account of their Habits, and a Description of two new Species, and with Notes on the Anatomy of some European Aquatic Species." By H. N. MOSELEY, M.A., Exeter College, Oxford. Communicated by G. ROLLESTON, M.D., Linacre Professor of Anatomy and Physiology in the University of Oxford. Received January 16, 1873.

(Abstract.)

The writer commences by expressing his great obligations to Professor Rolleston, whose pupil he formerly was. Professor Rolleston first informed him of the existence of Land-Planarians in Ceylon, and of the importance of investigating them. The paper was at first intended to be a joint one, and Professor Rolleston himself made a number of preparations of *Rhynchodemus*, one of which is figured. He likewise rendered great aid in the bibliography, and by constant suggestions and assistance during the progress of the work.

Two new species of Land-Planarians from Ceylon are described,—one belonging to the genus *Bipalium* (Stimpson), *B. Ceres*, the other to that of *Rhynchodemus*, *R. Thwaitesii*, so called after Mr. G. H. K. Thwaites, F.R.S., the illustrious curator of the Peradeniya Gardens, by whose assistance the specimens made use of were procured.

Lists are given of all the known species of *Bipalium* and *Rhynchodemus*, and also a map to show the distribution of *Bipalium* in space.

With regard to the habits of *Bipalium*, the most interesting facts noted are that these animals use a thread of their body-slime for suspension in air, as aquatic Planarians were observed to do for their suspension in water by Sir J. Dalyell, and the cellar-slug does for its suspension in air. The projection of small portions of the anterior margin of the head in the form of tentacles, originally observed by M. Humbert, becomes interesting in connexion with the discovery of a row of papillæ and ciliated pits in that region. The anatomy of the Planarians was studied by means of vertical and longitudinal sections from hardened specimens. The skin in *Bipalium* and *Rhynchodemus* closely conforms to the Planarian type, but is more perfectly differentiated histologically than in aquatic species, and approaches that of the leech in the distribution, colour, and structure of its pigment, and especially in the arrangement of the glandular system. The superficial and deep glandular system of the leech are both here represented. In *B. Ceres* peculiar glandular structures exist, which may foreshadow the segmental organs of Annelids, it being remembered that these segmental organs are solid in an early stage of development. Rod-like bodies (Stäbchen or stäbchenförmige Körper) are present in abundance, though, singularly enough, Max Schultze failed to find any in *Geoplana*. These rod-like bodies are probably homologous with the nail-like bodies of Nemertines; and it is possible that the setæ of Annelids are modifications of them. No light is thrown by the structure of these bodies in *Bipalium* on the question whether they are homologous with the urticating organs of Cœlenterata.

The muscular arrangement in *Bipalium*, which is very complex, throws great light on the homologies between the muscular layers of *Turbellaria* and those of other Vermes. It is commonly said that whilst in all other Vermes the external muscular layer is circular, and the longitudinal internal, in Turbellarians the reverse is the case. A wide gulf is thus apparently placed between these groups. In *Bipalium* there is an external circular muscular coat, which even presents the same imbricated structure which is found in it in leeches and other worms. In *Dendrocoelum lacteum* there is also an external circular coat. In cases where a distinct external circular muscular coat is absent, it is represented by a thick membrane, which is very probably contractile. The question resolves itself simply into a more or less perfect fibrillar differentiation of that membrane. All Turbellarians are built on the same essential type, as regards muscular arrangement, as are other worms. The general muscular arrangements in the bodies of the *Bipalium* and *Rhynchodemus* have become much modified from those of flat Planarians by the pinching together and condensation of the body, but they are nevertheless referable to the same type.

The digestive tract consists of three tubes (one anterior, two posterior),

as in other Planarians, and as in the embryo leech before the formation of the anus. Characteristic of land-Planarians, and consequent on the condensation of the body, is the absence of all diverticulæ from the inner aspects of the two posterior digestive tubes. This is found to be the case in *Geoplana*, *Bipalium*, *Rhynchodemus*, and *Geodesmus*. The close approximation of the intestinal diverticula in *Bipalium* and *Rhynchodemus*, and the reduction of the intervening tissue to a mere membranous septum, is very striking, and seems to foreshadow the condition of things in Annelids. The great difference in the form of the mouth in *Rhynchodemus* and *Bipalium* is also remarkable, considering the many points in which these forms are closely allied.

A pair of large water-vascular trunks, or, as they are here termed, primitive vascular trunks, are conspicuous objects in transverse sections of the bodies of *Bipalium* and *Rhynchodemus*. A peculiar network of connective tissue is characteristic of these vascular canals on section, and is shown to present exactly similar features in *Leptoplana tremellaris*, *Dendrocoelum lacteum*, and *Bothriocephalus latus*. The close agreement in the relative position of the oviducts to the vascular canals in *Dendrocoelum* and our land-Planarians is very remarkable. This primitive vascular system is homologous with the body-cavity present in the embryo leech and in *Branchiobdella* throughout life. It is not necessarily an excretory system, though the term water-vascular system has been generally considered to imply such a function for it. The nerves and ganglia of Planarians lie within the primitive vascular system, as do the corresponding structures within the primitive body-cavity of the leech.

Branches from the primitive vascular system in *Bipalium* serve to erect the penis, and probably supply the glandular tissue with fluid for secretion; others possibly proceed to the ciliated sacs in the head, and perform an excretory function. A small marine Planarian was found to contain hæmoglobin. In *Bipalium* there are a series of separate testes disposed in pairs, as in the leech. In *Rhynchodemus* the testicular cavities are more closely packed, and follow no such definite arrangement. The ovaries are simple sacs in both *Bipalium* and *Rhynchodemus*, and are placed very far forwards in the head, a long distance from the uterus. In *Bipalium* short branches given off from the posterior positions of the oviduct are the rudiments of a ramified ovary, such as exists in *Dendrocoelum lacteum*. There are also glands present, which probably represent the yolk-glands and shell-making glands of aquatic Planarians in a more or less rudimentary condition. There is a comparatively simple penis and female receptive cavity in both *Bipalium* and *Rhynchodemus*. In *Bipalium* there is, further, a glandular cavity at the base of the penis (prostate). The organs described as nervous ganglia by Blanchard in *Polycladus* are almost certainly its testes and ovaries; and therefore the arrangement of these bodies in *Polycladus* is the same as that in *Bipalium*.

The chain of nervous ganglia described as existing in *Bipalium* (*Sphyrocephalus*) by Schmarda, and which has been referred to by so many authors, does not exist. There is no doubt that Schmarda mistook the ovaries and testes for ganglia. The real nervous system is ill-defined, but appears to consist of a network of fibres without ganglion-cells, which lies within the primitive vascular canals. In *Leptoplana tremelaris* the structure of the ganglionic masses is remarkably complex in the arrangement of the fibres; and well-defined ganglion-cells of various sizes are present and have a definite arrangement.

Numerous eye-spots are present in *Bipalium*, most of them being grouped in certain regions in the head, but some few being found all over the upper surface of the body, even down to the tail. The eye-spots appear to be formed by modification of single cells. In *Rhynchodemus* two eyes only are present. All gradations would appear to exist between the simple unicellular eye-spot of *Bipalium* and the more complex eye of *Leptoplana* or *Geodesmus*, where the lens is split up into a series of rod-like bodies, forming apparently a stage towards the compound eyes of Articulata. It is quite probable that these compound eyes have arisen by such a splitting up into separate elements of a single eye, and not by fusion of a group of unicellular eyes, such as those of *Bipalium*. A peculiar papillary band runs along the lower portion of the margin of the head of *Bipalium*. The delicate papillæ are in the form of half cylinders, ranged vertically side by side. Between the upper extremities of the papillæ are the apertures of peculiar ciliated sacs. The papillæ, from the mode in which the animal makes use of them, are probably endowed with a special sense-function. The sacs may have a similar office, or they may be in connexion with the primitive vascular system, and have an excretory function; they may further be homologous with the ciliated tubes in Nemertines.

In considering the general anatomy of *Bipalium*, it is impossible to help being struck by the many points of resemblance between this animal and a leech. Mr. Herbert Spencer has, in his 'Principles of Biology,' placed a gulf between Planarians and Leeches by denoting the former as secondary, the latter as tertiary aggregates, so called because consisting of a series of secondary aggregates formed one behind the other by a process of budding. It is obvious, however, that a single leech is directly comparable to a single *Bipalium*. The successive pairs of testes, the position of the intromittent generative organs, the septa of the digestive tract, and, most of all, the pair of posterior cæca, are evidently homologous in the two animals. Further, were leeches really tertiary aggregates, the fact would surely come out in their development, or at least some indication of the mode of their genesis would survive in the development of some annelid. Such, however, is not the case. The young worm or leech is at first unsegmented, like a Planarian, and the traces of segmentation appear subsequently in it, just as do the protovertebræ in vertebrates

which Mr. Spencer calls secondary aggregates. If Mr. Spencer's hypothesis was correct, we should expect to find at least some Annelid developing its segments in the egg as a series of buds. It is not, of course, here meant to be concluded that Annelids are not sometimes in a condition of tertiary aggregation, as *Nais* certainly is when in a budding condition, but that ordinarily they are secondary and not tertiary aggregates; and if so, then so also are Arthropoda.

Much more information concerning the anatomy of Planarians will be required before it will be possible to trace the line of descent of *Bipalium* and *Rhynchodemus*, and determine what was the form of their aquatic ancestors. In the absence of accurate accounts of the structure of the American Land-Planarians, and even of the European *Rhynchodemus terrestris*, the question is very puzzling. The formation of either one of the two forms *Bipalium* or *Rhynchodemus* might be accounted for with comparative ease, from the arrangement of parts in the flat head of *Bipalium*. From the tree-like branching of the digestive tract in that region, the corresponding ramification of the vascular system, and general muscular arrangement, it might be imagined that *Bipalium* had come from a flattened parent of the common Planarian form, and that all the body except the head had become rounded and endowed with an ambulacral line. In nearly all points, except the eyes and the absence of branches to the oviduct, *Bipalium* seems more highly specialized than *Rhynchodemus*. We might imagine that *Rhynchodemus* and *Bipalium* had a common parent, and that when an ambulacral line was just beginning to be developed, the two forms took different lines—*Rhynchodemus* losing all traces of the original flatness of its ancestor, and never developing any ciliated sacs or papillæ, but cherishing a single pair of large eyes at the expense of all the rest which it possessed, its testes, moreover, remaining in a comparatively primitive condition. But then comes the difficulty about the great difference in shape in the pharynxes of the two forms; and if it be suggested that, as is highly probable, several or many aquatic Planarians have taken to terrestrial habits, and that *Bipalium* has been derived from a form like *Leptoplana*, with a folded pharynx, whilst *Rhynchodemus* came from an ancestor with a tubular one, it is difficult to account for the many points of close resemblance between these two forms, and especially their similarity in external colouring, though this latter may perhaps be explained by mimicry. On the whole, it is evident that a close study of the anatomy of Land-Planarians cannot fail to lead to interesting results, and it is hoped that this memoir may lead to further work of the same kind. It would be of especial value to have a good account of the anatomy of *Geodesmus* and *Rhynchodemus sylvaticus*.

- II. "On a new Locality of Amblygonite, and on Montebasite, a new Hydrated Aluminium and Lithium Phosphate." By A. O. DES CLOIZEAUX. Communicated by Prof. W. H. MILLER, Foreign Secretary R.S. Received November 27, 1871.

(Abstract.)

A mineral found in 1862 at Hebron, Maine, U.S. A., after a mere tentative examination by Professor Brush, who announced the presence in it of lithia in considerable quantity, resembled the amblygonite of Penig so closely as to lead to its being looked on as amblygonite. The crystalline system and birefringent optical characters of this mineral were determined by the author in 1863. In 1870 a mineral found in the tin vein of Montebas (Creuse), though resembling the amblygonite of Hebron, appeared to the author to differ from it so far as to justify his designation of it under the name of Montebasite. Towards the close of 1871 he received another specimen from Montebas, which presented all the characters of the American amblygonite, and which consequently was easily distinguished from the montebasite. Subsequently, analyses by Pisani, v. Kobell, and Rammelsberg, and optical observations by the author, proved the identity of the montebasite of Montebas with the amblygonite from Penig. But this is not the case with the amblygonite from Hebron, nor with that from Montebas, which had been analyzed by Pisani. These differ from the amblygonites of Saxony and Montebas (which last he had previously named montebasite) by the absence of soda, by the preponderance of lithia, and the presence of a notable amount of water, while at the same time they contain almost equal proportions of phosphoric acid and alumina.

The differences which these two minerals present in their physical and chemical characters are sufficiently decided to compel our treating them as distinct species. The name amblygonite should be retained for the sodolithic species first discovered at Penig by Breithaupt, and the white or violet-tinted lamellar masses abundant at Montebas will be included under it; the hydrated and entirely lithic species comprising the laminar specimens and the crystals from Maine, as well as some greenish masses from Montebas, should be embraced under the name montebasite.

The amblygonite of Montebas has only been met with in laminar masses with a faint tinge of violet. These masses exhibit two cleavages presenting nearly the same degree of facility, making with one another an angle of $105^{\circ} 44'$. Close observation shows that the sharpness of the reflected images is generally a little greater on one of the cleavages than on the other; and this induces one to suppose that they do not both belong to equivalent crystallographic planes. The study of some of their optical properties, though presenting certain special difficulties, arising from the small extent of the transparent portions and the presence of

numerous twin plates, even in the specimens that to all appearance are the most homogeneous, has proved that the laminar masses of montebrasite must be referred to the triclinic system. The optic axes are situated in a plane which divides into two very unequal parts the acute angle of $74^{\circ} 16'$ of the two cleavages. This direction is entirely different from that found for montebrasite of Hebron and of Montebras, in which the plane of the axes lies in the obtuse angle of 105° formed by the two principal cleavages.

The appearance of the bars traversing the central ring of each system indicates very distinctly a twisted dispersion, as well as a small amount of inclined dispersion, which is characteristic of a crystal belonging to the triclinic system.

In November 1871 the author received a specimen from the middle of a mass of amblygonite from Montebras resembling the mineral from Hebron. It has three principal cleavages, p , m , t , which the author recognized in the mass from Hebron, the angles between which are $p\ m=105^{\circ}$, $m\ t=135^{\circ}$ to 136° , $p\ t=89^{\circ}$ to $89^{\circ} 15'$.

By means of artificial twins formed of two plates, each of which had been worked perpendicular to the two cleavages p and m , and which were united by their faces p , it appeared that the plane of the optic axes is situated in the obtuse angle $p\ m$, and traverses the edge $\frac{p}{m}$, but that it is not quite normal to m , since it gives angles of about 82° with m and 23° with p . The character of the coloured rings shows that in montebrasite of Montebras, as in that from Hebron, there coexists with the horizontal a well-marked inclined dispersion; and these are peculiar to crystals of the triclinic system.

Analyses by M. Pisani.

	Hebron.	Montebras.
Fluorine.....	5.22	3.80
Phosphoric acid.....	46.65	47.15
Alumina.....	36.00	36.90
Lithia.....	9.75	9.84
Water	4.20	4.75
	<hr/> 101.82	<hr/> 102.44
Specific gravity	3.03, Pisani.	3.01, Pisani.
	2.99, Damour.	2.977, Damour.

Wavellite, in the form of thin coatings, forms a layer over almost all the fissures that occur in the amblygonite of Montebras. In cavities in these coatings are found long thin needles, which have enabled the author to correct the older measurements of this mineral.

M. Pisani has ascertained that the variety from Montebias yields :—

Fluorine	2.27
Phosphoric acid	34.30
Alumina	38.25
Water	26.60

101.42

Specific gravity 2.33

February 27, 1873.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read :—

“On Leaf-Arrangement.” By HUBERT AIRY, M.A., M.D. Communicated by CHARLES DARWIN, F.R.S. Received January 21, 1873.

(Abstract.)

Assuming, as generally known, the main facts of leaf-arrangement,—the division into the whorled and spiral types, and in the latter more especially the establishment of the convergent series of fractions, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, $\frac{5}{13}$, $\frac{8}{21}$, $\frac{13}{34}$, $\frac{21}{55}$, $\frac{34}{89}$, $\frac{55}{144}$, &c., as representatives of a corresponding series of spiral leaf-orders among plants,—we have to ask, what is the meaning that lies hidden in this law ?

Mr. Darwin has taught us to regard the different species of plants as descended from some common ancestor ; and therefore we must suppose that the different leaf-orders now existing have been derived by different degrees of modification from some common ancestral leaf-order.

One spiral order may be made to pass into another by a twist of the axis that carries the leaves. This fact indicates the way in which all the spiral orders may have been derived from one original order, namely by means of different degrees of twist in the axis.

We naturally look to the simplest of existing leaf-orders, the two-ranked alternate order $\frac{1}{2}$, as standing nearest to the original ; for it is manifest that the orders at the other extreme of the series (the condensed arrangement of scales on fir-cones, of florets in heads of *Compositæ*, of leaves in close-lying plantains, &c.) are special and highly developed instances, to meet special needs of protection and congregation : they are, without doubt, the latest feat of phyllotactic development ; and we may be sure that the course of change has been from the simple to the complex, not the reverse. This point will be illustrated by experiment below.

But first, what are the uses of these orders? and at what period of the leaf's life does the advantage of leaf-order operate? The period must be that at which the leaf-order is most perfect: not, therefore, when the twig is mature, with long internodes between the leaves, but while the twig and its leaves are yet *in the bud*; for it is in the bud (and similar crowded forms) that the leaf-order is in perfection, undisturbed by contortions or inequalities of growth; but, as the bud develops into the twig, the leaves become separated, the stem often gets a twist, the leaf-stalks are curved and wrung to present the blades favourably to the light, and thus the leaf-order that was perfect in the bud is disguised in the grown twig.

In lateral shoots of *yew* and *box* and *silver fir* we see how leaves will get their stalks twisted to obtain more favourable exposure to light; and if general distribution round the stem were useful to the adult leaves, we should expect the leaves of a vertical *elm*-shoot (for example) to secure such distribution by various twists of stalk and stem; but the leaf-blades of the elm keep their two ranks with very great regularity. This goes to show that it is not in the mature twig that the leaf-order is specially advantageous.

In the *bud* we see at once what must be the use of leaf-order. It is for *economy of space*, whereby the bud is enabled to retire into itself and present the least surface to outward danger and vicissitudes of temperature. The fact that the order $\frac{1}{2}$ does not exhibit this advantage in any marked degree, supports the idea that this order is the original from which all the more complex spiral orders have been derived.

The long duration of the bud-life as compared with the open-air life of the leaf gives importance to the conditions of the former. The open-air life of the bud is twelve months, and adding the embryo life of the bud, we have about a year and a half for the whole life of the bud; and for the twelve months of its open-air life it is in a state of siege, against which a compact arrangement of its embryo-leaves within must be of great value. But the open-air life of the unfolded leaves is (except in evergreens) not more than six months.

That the order $\frac{1}{2}$ would under different degrees of contraction (with twist) assume successively the various spiral orders that exist in nature, in the order of their complexity, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, $\frac{5}{13}$, &c., may be shown by the following experiment:—

Take a number of spheres (say oak-galls) to represent embryo leaves, and attach them in two rows in alternate order ($\frac{1}{2}$) along opposite sides of a stretched india-rubber band. Give the band a slight twist, to determine the direction of twist in the subsequent contraction, and then relax tension. The two rows of spheres will roll up with a strong twist into a tight complex order, which, if the spheres are attached in close contact with the axis, will be nearly the order $\frac{1}{3}$, with three steep spirals. If the

spheres are set a little away from the axis, the order becomes condensed into (nearly) $\frac{2}{3}$, with great precision and stability. And it appears that further contraction, with increased distance of the spheres from the axis, will necessarily produce the orders (nearly) $\frac{2}{3}$, $\frac{5}{8}$, $\frac{8}{11}$, &c. in succession, and that these successive orders represent successive *maxima* of stability in the process of change from the simple to the complex.

It also appears that the necessary sequence of these successive steps of condensation, thus determined by the geometry of the case, does necessarily exclude the non-existent orders, $\frac{1}{2}$, $\frac{3}{4}$, $\frac{5}{6}$, $\frac{7}{8}$, $\frac{9}{10}$, &c.

Numbering the spheres from 0 upwards, it appears that, under contraction, the following numbers are brought successively into contact with 0, alternately to right and left:—1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c. None of them stands vertically above 0 while in contact with it, but a little to the right or a little to the left; and so far the results of this experiment fall short of the perfect fractions $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{5}{8}$, &c.: but in this very failure the results of the experiment are more closely in agreement with nature than are those perfect fractions themselves; for those fractions give the angular divergence only in round numbers (so to speak), and lose account of the little more, or the little less, which makes all the difference between a vertical rank and a spiral. In the large majority of spiral-leaved plants, one has to be content with “ $\frac{2}{3}$ nearly” or “ $\frac{3}{5}$ nearly,” and it is difficult to find a specimen in which the fraction represents the order exactly.

The geometrical relations of the members of the above series 1, 2, 3, 5, 8, 13, &c. are as simple as their numerical relations.

Analysis of the order seen in the head of the sunflower and other examples, by consideration of their several sets of spirals, presents a striking agreement with the above synthetical process. In the sunflower, a marginal seed taken as 0 is found to be in contact with the 34th, the 55th, and the 89th (counted in order of growth), and even with the 144th, if there is not contact with the 34th. The dandelion, with a lower degree of condensation, has 0 in contact with the 13th, the 21st, and the 34th in large specimens; the house-leek in its leaf-order has 0 in contact with the 5th, 8th, and 13th; the apple-bud has 0 in contact with the 2nd, 3rd, and 5th; and thus we see that in nature the very same series of numbers is found to have contact-relation with 0, which we have already seen possessing that relation in the experimental condensation of the order $\frac{1}{2}$.

Difference of leaf-order in closely allied species (e.g. *Plantago major* and *P. coronopus*) is found in close relation to their different habits and needs.

The prevalence of the order $\frac{1}{2}$ in marine *Algæ*, and in *Gramineæ*, a low-developed gregarious group, and its singular freedom from individual

variation in that group and in elm, beech, &c., support the view that this order is the original of the spiral orders.

In many plants we find actual transition from the order $\frac{1}{2}$ to an order more complex, as, for instance, in *Spanish chestnut*, *laurels*, *nut*, *ivy*; and these instances agree in presenting the complex order in the buds that occupy the most exposed situations, while they retain the simple $\frac{1}{2}$ in the less exposed lateral buds. Several kinds of *aloe* have the order $\frac{1}{2}$ in their basal leaves and a higher order in the remainder. A species of *cactus* often contains a complete epitome of phyllotaxy in a single plant or even in a single shoot.

Shoots of *acacia* often present a zigzag disposition of their leaves, on either side of the branch, which seems unintelligible except as a distortion of an original two-ranked order.

The prevalent two-ranked arrangement of rootlets or roots seems to be a survival underground of an order which originally prevailed through the whole plant, root, stem, and branch.

In the whole Monocotyledonous class the first leaves in the seed have the order $\frac{1}{2}$.

In the Dicotyledonous class the first leaves in the seed have the simplest order of the whorled type.

As the spiral orders have probably been derived from a two-ranked alternate arrangement, so the whorled orders have probably been derived from a two-ranked *collateral* (two abreast) arrangement. This is illustrated by an experiment similar to the former; and it is seen that successive parallel horizontal pairs of spheres are compelled under contraction to take position at right angles to one another, exactly in the well-known crucial or decussate order. These whorls of two contain potentially whorls of three and four, as is seen in variations of the same plant; but the experiment does not show the change.

The reason of the non-survival of the (supposed) two-ranked *collateral* order lies in its manifest instability; for under lateral pressure it would assume the alternate, and under vertical the crucial order.

The bud presents in its shape a state of equilibrium between a force of contraction, a force of constriction, and a force of growth.

To sum up, we are led to suppose that the original of all existing leaf-orders was a two-ranked arrangement, somewhat irregular, admitting of two regular modifications, the alternate and the collateral; and that the alternate has given rise to all the spiral orders, and the collateral to all the whorled orders, by means of advantageous condensation in the course of ages.

Presents received February 6, 1873.

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“On the Combined Action of Heat and Pressure upon the Paraffins.” By T. E. THORPE and JOHN YOUNG, of the Andersonian University, Glasgow. Communicated by Prof. H. E. Roscoe, F.R.S. Received June 5, 1872*.

1. In a preliminary communication read before the Royal Society on March 9th, 1871†, we described the combined effect of heat and pressure upon the solid paraffins. We showed that when these substances are exposed to a high temperature in a closed vessel they are almost completely resolved, with the evolution of but little gas, into hydrocarbons, which remain liquid at the ordinary temperature. This transformation may be easily effected on the small scale. A few grms. of ordinary paraffin are sealed up in a piece of strong combustion-tubing bent in the form of the letter V: the tube is securely surrounded by stout wire gauze, and the limb containing the paraffin is gently heated along its entire length in a gas combustion-furnace. If the heat is properly regulated, the paraffin rapidly distils over and solidifies in the cold portion of the tube. The gas-flames are then turned down, the tube reversed, and the paraffin again distilled. After a very few repetitions of this process the paraffin acquires the consistency of butter, and the warmth

* Read June 20, 1872. See abstract, vol. xx. p. 488.

† Proc. Roy. Soc. vol. xix. p. 370.

of the hand is sufficient to liquefy it, and after about a dozen distillations it remains permanently liquid. Although there is no difficulty in thus ultimately effecting the resolution of the solid paraffin into liquid products, the rapidity of the transformation seems to be dependent upon conditions which we cannot yet say we have satisfactorily determined. On one occasion, after all the paraffin had been distilled over and had solidified in the colder limb of the tube, the empty portion in contact with the flame became softened and blew out, when instantly, on the removal of the pressure, the solid matter became liquid, and slightly effervesced as if gas had been condensed in the substance by the pressure to which it had been subjected. It seems to be absolutely necessary that the paraffin should be thus distilled over and condensed; by merely heating it in the tube in such a manner that the condensed vapours flow back again upon the heated portion, the liquefaction of the paraffin is never accomplished. About 6 grms. of a pure paraffin, melting at $41^{\circ}5$ C. (see section 12), were heated in a sealed tube to about 200° for twelve hours, but the melting-point remained unchanged. The same portion of paraffin was then transferred to a V-tube surrounded by wire gauze, and distilled backwards and forwards in the combustion-furnace. After six distillations it was rendered completely liquid. The total quantity of liquid obtained was about 6 cub. centims. On gently warming it, bubbles of gas were evolved, and the liquid was in full ebullition at 40° , but only about one sixth came over below 100° ; about $2\frac{1}{2}$ cub. centims. distilled between 100° and 200° , and nearly the whole of the remainder below 300° ; the small quantity remaining solidified when the bulb was plunged into cold water.

2. It appears that only paraffins boiling at an extremely high temperature, and those usually solid under ordinary conditions, are thus susceptible of decomposition. The readiness with which they yield liquid hydrocarbons appears to depend upon the complexity of their constitution. We have not determined with certainty the limits of the decomposition; but we find that the paraffin (and olefine) boiling at about 255° (see section 11) may be repeatedly distilled backwards and forwards in a sealed V-tube without suffering the slightest change. 6-7 grms. of the mixture of hydrocarbons were distilled backwards and forwards twenty-one times; but on opening the tube no gas was evolved, and the boiling-point (252° - 255° uncorrected) remained unaltered. A further proof of its unalterability on repeated distillation under pressure is afforded by its reaction with bromine (see section 5).

I. 9.38 grms. of the mixed hydrocarbons before distillation required 2.95 grms. of bromine before the liquid became permanently reddened; 100 parts by weight would require 31.4.

II. 8.605 grms. hydrocarbon before distillation required 2.672 grms. of bromine, or 100 grms. would require 31 of bromine. Mean amount of bromine needed 31.2 per cent.

III. 5.13 grms. of the hydrocarbon after repeated distillation (21 times) required 1.54 grm. of bromine, or 100 grms. would require 30 grms.

3. In the hope of throwing some light upon the constitution of the solid paraffins, we have repeated this process of transformation into liquid products upon a large scale. The paraffin employed was obtained from shale; it melted at 46° and solidified at 43° , and had a specific gravity of 0.906 at 13° , when solidified under an extra pressure of 0.75 millim. of mercury*.

It was burnt with copper oxide in a stream of oxygen, with the following results:—

I. 0.2980 grm. paraffin gave 0.9293 grm. carbon dioxide and 0.3980 grm. water.

II. An unknown quantity gave 1.4245 grm. carbon dioxide and 0.6060 grm. water.

	I.	II.	Mean.
Carbon	85.05	85.23	85.14
Hydrogen	14.84	14.77	14.81
	<hr/> 99.89	<hr/> 100.00	<hr/> 99.95

* Considerable discordances exist in the statements by various observers of the specific gravities of the solid paraffins. These differences are doubtless due more to the difficulty of obtaining the bodies perfectly homogeneous after melting than to any great variations in their true specific gravity. When a piece of ordinary paraffin is examined, it is seen to be made up of an infinite number of small cavities; and however carefully it may be cooled after melting, the interstices are invariably present. From this circumstance it is obvious that any attempt to determine the real specific gravity of this body would give too low a result. By solidifying the paraffin under pressure, the formation of the cavities may be, to a great extent, obviated. We have effected the solidification in the apparatus figured in the margin. Mercury was first poured into the tube up to the level A B, the bottom end of the tube being securely plugged by a cork C. The wider portion D of the tube was then filled with the melted paraffin, and a well-fitting caoutchouc cork fastened down by wire into the opening, care being taken to exclude all bubbles of air. Mercury was now quickly poured into the longer limb of the tube until it stood about 0.8 m. above the level of the metal in the wider tube, and under the pressure of this mercury the paraffin was allowed to solidify slowly. By withdrawing the cork C, the mercury could flow out, and on warming the sides of the tube the plug of paraffin was easily detached. Thus solidified, the body was almost entirely free from cavities, and portions perfectly homogeneous were easily found; these had a specific gravity of 0.906 at 13° .

These numbers agree perfectly with those obtained by Anderson many years ago* from analysis of a paraffin identical in origin and melting-point ($45^{\circ}5$) with that employed by us. Dr. Anderson found for the substance the composition :—

Carbon.....	85.1
Hydrogen	15.1
	<hr/>
	100.2

A sample of paraffin from peat, melting at $46^{\circ}7$, gave :—

	I.	II.	Mean.
Carbon	84.95	85.23	85.09
Hydrogen	15.05	15.16	15.10
	<hr/>	<hr/>	<hr/>
	100.00	100.39	100.19

In the process of conversion we replaced the bent glass tube above described by an apparatus consisting of two cast-iron mercury bottles connected together by a bent iron pipe, in which were fixed a stopcock and pressure-gauge. One of the bottles was charged with the paraffin, and was heated over an ordinary coal fire, the heat being so regulated that a pressure of 20 to 25 lbs. was maintained in the apparatus throughout the operation. The volatilized products distilled over into the second bottle, which acted as a condenser. In about four or five hours the operation was concluded, and the distillate had the appearance of a magma of oil, and of apparently unaltered paraffin. This mixture melted at a comparatively low temperature, the warmth of the hand being sufficient to render it completely liquid.

Three and a half kilograms of the paraffin yielded about 4 litres of liquid hydrocarbons. On distillation the mixture commenced to boil at 18° , but the quantity coming over below 100° was comparatively small; by far the greater portion boiled between 200° and 300° . A preliminary separation showed that the 4 litres were approximately made up of hydrocarbons boiling :—

	litres.
I. Below 100°	0.3
II. From 100 – 200°	1.0
III. From 200 – 300°	2.7
	<hr/>
	4.0

A considerable quantity of substance, which could not be distilled within the range of the mercurial thermometer, remained in the retort, and solidified on cooling (see section 12). Each of the portions was then submitted to a systematic fractional distillation.

* Rep. Brit. Assoc. 1846.

4. *Fraction boiling below 100°*.—After repeated distillations over sodium this portion was almost completely resolved into three fractions, boiling constantly at (1) 32–38°, (2) 65–70°, and (3) 94–97°. The quantity of liquid boiling below 30° did not amount to 5 cub. centims. Although the distillation was conducted in winter, and freezing-mixtures of snow and salt were employed for the condensation, the greater portion of the product boiling at 32–38° came over at about 35°; this boiling-point would indicate that the liquid is probably either *quintane* or *amylene*, or a mixture of these hydrocarbons. In order to decide the point, the distillate was treated with bromine. The liquid was violently attacked, and each drop of bromine combined with the hydrocarbon with a hissing noise; so energetic was the reaction, that, in order to moderate it, it was necessary to immerse the hydrocarbon in a freezing-mixture. When proper care was taken to cool the liquid, scarcely a trace of hydrobromic acid was evolved. In order to prevent the possible formation of higher brominated products, the addition of the bromine was from time to time interrupted, and the liquid submitted to distillation; it invariably commenced to boil at about 32°, and the distillation was maintained until the thermometer stood at 50°. The distillate was again treated with bromine in the cold, and again distilled, the process being repeated until it was without visible action upon the hydrocarbon,—that is, until the liquid was permanently reddened. It was then washed with dilute potash solution, and rectified over sodium; it boiled constantly at 35–37°; the liquid was evidently *quintane*, and in all probability normal *quintane*, the boiling-point of which is given by Schorlemmer at 37–39°, and by Warren at 37°. The amount was far too small to attempt the preparation of any derivatives from it. The brominated portion boiling above 50° was then distilled; the thermometer rose rapidly to 180°, and the whole of the liquid came over below 200°. After renewed distillation the liquid was found to boil constantly, but with slight decomposition, at 184–188°; this compound is *amylene dibromide*, the boiling-point of which, according to Wurtz, is in the neighbourhood of 180°.

0.6515 grm. bromide gave 1.0125 grm. Ag Br and 0.0235 grm. reduced silver.

	Calculated.	Found.
Br.....	69.5	68.8

5. The fraction boiling between 65° and 70°, which was at least fifty times greater than that boiling at 32–38°, was then treated with bromine; the bromine was absorbed with the same avidity as in the preceding case; and it was equally necessary to cool the hydrocarbon in order to moderate the reaction. As soon as the liquid appeared permanently reddened, it was distilled. The portion boiling below 80° was no longer *instantly* attacked by bromine; it was accordingly free from the accompanying

olefine. The distillate was then treated with aqueous potash to remove the free bromine, and when colourless the hydrocarbon was decanted, dried, and distilled over sodium; it boiled almost entirely between 65° and 70°, by far the greater portion coming over at 67–68°. It was analyzed with the following results:—

0.1440 grm. hydrocarbon gave 0.2095 grm. water; the carbon determination was lost.

	Calculated.	Found.
C ₈	83.72	—
H ₁₄	16.28	16.16
	100.00	

Two determinations of its specific gravity made by means of different bottles gave:—

- (1) 0.6620 at 19°.5, and
- (2) 0.6641 at 18°.

Compared with water at the same temperature, Pelouze and Cahours found 0.669 at 16° for the hexane boiling at 68° obtained from American petroleum. Hexane from suberic acid, boiling at 69°.5, according to Dale, has a specific gravity of 0.6617 at 17°.5; this agrees with that of β hexane obtained from mannite, viz. 0.6645 at 16°.5. Schorlemmer found 0.678 at 15°.5 for the hydride from cannell coal, which agrees perfectly with that obtained by Williams for the same hydride prepared from boghead coal, viz. 0.6745 at 18°. On the other hand, Schorlemmer has found that the hexane prepared from mannite is probably identical with that obtained for secondary hexyl iodide by the action of zinc and hydrochloric acid, and with the dipropyl obtained from primary hexyl iodide, for which he found the specific gravity 0.6630 at 17°; and these are probably identical with the hexane of Pelouze and Cahours obtained from petroleum, and with the hydride found by Schorlemmer in cannell oil, all being normal hydrocarbons with the constitution



The brominated hydrocarbon left in the flask after removing the hexane was then distilled: it commenced to boil at about 190°; the thermometer gradually rose to 195°, and nearly the whole of the liquid distilled over below 205°. As the compound showed signs of decomposition on boiling, hydrobromic acid being continuously evolved during the distillation, a considerable amount of tarry matter remaining in the flask, it was impossible to fractionate it further. It was purified, however, by distillation in a current of steam; it was nearly colourless, and on analysis yielded numbers agreeing with the formula C₈H₁₂Br₂.

0.8950 grm. of the bromide gave 1.3570 grm. silver bromide and 0.0090 grm. reduced silver.

	Calculated.	Found.
Br.....	65.57	65.28

The following are the data obtained in the determination of its specific gravity:—

Weight of bottle	1.6807 grm.
Bottle + water at 20°	7.2942 grms.
Bottle + bromide at 20°	10.6440 „

from which 1.5967 is obtained as the specific gravity of hexylene dibromide at 20° compared with water at the same temperature.

A second determination with a smaller quantity gave 1.5975 at 18°. Pelouze and Cahours found the boiling-point of this body to be 192–198°, and its specific gravity 1.582 at 19°. These numbers are certainly too low; the bromide had probably been slightly decomposed by distillation.

We have attempted to determine the relative amounts of hydride and olefine in the fraction boiling at 65–68° by means of the reaction with bromine.

The hydrocarbon was weighed out in a small distillation-flask, and well cooled by a mixture of ice and salt; bromine was then slowly added, and in minute drops, by means of the little apparatus seen in the figure—a small caoutchouc ball about 5 cub. centims. in diameter, which can be compressed by the plate movable along the rods by means of the milled head-screw. The mode of working the instrument is obvious; by means of it the bromine can be easily transferred from the bottle in which it is preserved to the hydrocarbon, and the amount added can be regulated with great nicety. The bromine was added until the liquid was permanently reddened, when the flask was again weighed to determine the amount of bromine employed. 13.30 grms. of the mixed hydrocarbons boiling at 65–70° required 13.16 grms. bromine before the red colour was persistent. This amount of bromine is equivalent to 6.9 grms. of hexylene, and $13.3 - 6.9 = 6.4$ as the amount of hexane present. The mixture was now distilled, and all passing over below 110° (at which point the liquid in the flask commenced to blacken) collected in a weighed test-tube. The weight of the distillate was 6.18 grms. From these numbers it would appear that the product boiling at 65–68° is a mixture of hexane and hexylene in about equal proportions.

6. The fraction boiling at 94–97° was next treated with bromine in the cold until the liquid was reddened. The termination of the reaction was not so easily seen in this case; the mixture blackened slightly, and more hydrobromic acid was evolved, although the hydrocarbon was carefully cooled by ice and salt. The brominated mixture on distillation commenced to boil about 95°, and it was heated to 130°, when the liquid in the flask commenced to decompose; at this point, therefore, the distillation was interrupted. The following are the details of a quantitative determination:—

Weight of mixed hydrocarbons	14.21	grms.
Weight of bromine added	12.9	"
Equivalent to heptylene.....	7.9	"
Equivalent to heptane	6.3	"
Amount of heptane actually obtained ..	6.15	"

From this experiment it would appear that the amount of olefine is slightly greater than that of the hydride in the fraction boiling at 94–97°; but, from the circumstances of the experiment, we have reason to believe that the amount of heptylene thus indicated is slightly above the truth. It was difficult to determine the final point of the reaction; the hydride was not altogether unacted on by the bromine, and small quantities of hydrobromic acid were continuously disengaged during the process; these circumstances would tend to increase the weight of bromine used, and consequently the apparent weight of the olefine present. In extending our research to the determination of the constitution of these hydrocarbons, we have had occasion to repeat this experiment on a far larger scale and under more favourable conditions.

900 grms. of the mixed heptane and heptylene required 716 grms. of bromine; this would be equivalent to 439 grms. of heptylene. 900 grms. of a mixture of heptane and heptylene in equivalent proportions would contain 444 grms. of heptylene.

The heptylene bromide could not be redistilled under the ordinary atmospheric pressure without decomposition; it commenced to blacken at about 150°, and dense fumes of hydrobromic acid were evolved. When distilled in a current of steam, it was obtained nearly colourless.

It cannot be preserved, however, without slight decomposition; when exposed to light, it slowly darkens in colour and evolves hydrobromic acid gas.

It was analyzed with the following results:—

0.7470 grm. gave 1.0210 grm. Ag Br and 0.035 grm. reduced silver.

	Calculated for $C_7H_{14}Br_2$	Found.
Br.....	62.01	61.66

The specific gravity of this compound was found to be 1.5146 at 18°.5, water at the same temperature being taken as unity.

The heptane, after washing with dilute potash solution, drying, and rectification over sodium, boiled constantly at 97–99°. Its specific gravity was found from two experiments to be (1) 0.6910 at 19°, and (2) 0.6915 at 18°. Warren and Storer found for the same hydrocarbon from Menhadden oil the specific gravity of 0.6942 at 17°.5.

Burnt with copper oxide in a stream of oxygen, it yielded the following numbers:—

I. 0.1695 grm. hydride gave 0.5240 grm. carbon dioxide and 0.2460 grm. water.

II. 0.2638 grm. hydride gave 0.8119 grm. carbon dioxide and 0.3733 grm. water.

III. An unknown quantity gave 0.5015 grm. carbon dioxide and 0.2318 grm. water.

	Calculated.	Found.		
		I.	II.	III.
C ₇	84.00	84.29	83.94	84.14
H ₁₀	16.00	16.13	15.73	15.86
	<hr/> 100.00	<hr/> 100.42	<hr/> 99.67	<hr/> 100.00

These six bodies, quintane and amylene, hexane and hexylene, heptane and heptylene, are the only liquids we have been able to isolate from the fraction boiling below 100°. Careful search was made for the other hydrocarbons, particularly for benzol and for the hexylene of Caventou, boiling about 80°, but without success. The fractions intermediate between 70° and 90° could be resolved by repeated distillations into products boiling at 65–70° and 90–100°. The amount coming over in the neighbourhood of 80° was very small; in fact the entire quantity eventually obtained between 70° and 90° amounted to only a few grams. About a gram of the small portion boiling at 80–82° was treated with strong nitric acid; the liquid was very slowly acted upon, and when treated with alcohol, zinc, and hydrochloric acid, after washing and solution in ether, it failed to give the characteristic reaction for aniline; nor on treating the remainder of the hydrocarbon with excess of bromine, and allowing the mixture to evaporate, could any solid bromide (C₆H₁₀Br₂) be obtained. Hence we infer the absence of members of the C_nH_{2n-2} and C_nH_{2n-6} groups in the liquid hydrocarbons obtained by the decomposition of solid paraffins.

7. *Fraction boiling between 100° and 200°.*—As this portion amounted to about a litre, it was thought worth while to attempt a more rigorous separation of the hydrocarbons than is possible by the ordinary method of fractionation; for this purpose a small condensing arrangement on Warren's principle was constructed, and by means of it the hydrocarbons were separated at first at intervals of 10°, afterwards into portions boiling between 5°, and eventually into portions boiling between 2°. The hydrocarbons were distilled over sodium, and the temperature of the worm was maintained about 20° lower than that of the vapour in the flask. A considerable quantity of liquid was obtained boiling below 100°; and this could be resolved into hexane and hexylene, heptane and heptylene. The amount of liquid eventually obtained between 100° and 120° was very small.

About 120 grms. of liquid were found to boil constantly between 122° and 125°. Bromine was eagerly absorbed by this fraction, indicating

the probable presence of octylene, the boiling-point of which is given by Bouis at 125°, and by Pelouze and Cahours at 118–120°. The reaction with bromine yielded the following data:—

Amount of hydrocarbon taken	41·0 grms.
Amount of bromine needed	84·0 „
Equivalent of octylene	24·1 „

On the addition of bromine the hydrocarbon darkened rapidly in colour, and a small quantity of tarry matter collected at the bottom of the flask. As it was impossible to distil the mixture under the ordinary atmospheric pressure, the bromide being decomposed by a very slight elevation of temperature, it was treated with alcoholic potash, and distilled *per ascensionem* for some hours until a few drops thrown into water floated upon its surface. Water was then added, the lighted portion separated, dried, and fractionated. The largest amount came over between 120° and 140°, the next fraction from 140–160° being much smaller in quantity, whilst scarcely any liquid distilled over between 160° and 180°. After this point the thermometer became more stationary, and a fraction almost as large as that at 120–140° came over between 180° and 200°. This, on renewed distillation, boiled at 185–190°; it had a feeble garlic smell, and was heavier than water. Analysis showed that it had approximately the composition $C_8H_{16}Br$.

I. 0·4755 grm. bromide gave 0·4165 grm. silver bromide and 0·0180 grm. reduced silver.

II. 0·4600 grm. bromide gave 0·4100 grm. silver bromide and 0·0150 grm. reduced silver.

	Calculated.	Found.	
		I.	II.
Br	41·8	40·1	40·7

According to Rubien*, by whom this compound was first described, its boiling-point is 185°.

The fraction boiling between 120° and 140° on repeated distillation was separated into octane, boiling at 122–125°, and a liquid of an alliaceous odour boiling constantly at 130–135°, which absorbed bromine with avidity. This in all probability was caprylidene, the boiling-point of which has already been found by Rubien (*loc. cit.*) to be 133–134°; its formation was doubtless due to the prolonged action of the alcoholic potash on the $C_8H_{16}Br$.

Action of chlorine on mixed hydrocarbons boiling at 122–125°.—34 grms. of the hydrocarbons were weighed out into a small distillation-flask, immersed in a mixture of ice and salt, and a gentle stream of chlorine sent through the liquid in the dark. The chlorine was rapidly absorbed, and for the first hour but little hydrochloric acid was disengaged; during

* Ann. Ch. Pharm. vol. cxiii. p. 204.

the second hour and to the close of the operation, the evolution of this gas increased largely, and the mixture darkened considerably.

The chlorinated liquid was then distilled in a current of steam; as soon as the distillate appeared to be heavier than water the receiver was changed, and the heavy liquid separated, dried, and distilled. It commenced to boil at about 220° , but showed no constant boiling-point; but the greater portion came over between 230° and 240° . At this high temperature the product suffered rapid decomposition. The portion boiling at 230 – 240° was redistilled in a current of steam. After drying over calcium chloride it was nearly colourless, heavier than water, and of a pleasant aromatic odour. Analysis showed that it was *octylene dichloride*.

0.2760 grm. chloride gave 0.4225 grm. silver chloride and 0.0100 grm. reduced silver.

	Calculated.	Found.
Cl	38.80	39.09

The portions of octane obtained after treatment with chlorine and with bromine were then mixed and repeatedly distilled over sodium. The mixed liquid commenced to boil at 118° , and about two fifths came over below 122° , the remainder distilling between 122° and 125° . Both fractions were immediately reddened by bromine vapour. The 122 – 125° fraction was analyzed with the following results:—

0.2970 grm. hydrocarbon gave 0.9170 carbon dioxide and 0.4095 grm. water.

	Calculated.	Found.
C ₈	84.22	84.20
H ₁₈	15.78	15.32
	<hr/> 100.00	<hr/> 99.52

The specific gravity of this fraction was found to be 0.7207 at $15^{\circ}5$; the fraction 118 – 122° had the specific gravity 0.7165 at $15^{\circ}6$. Zincke found 0.7124 at 16° for normal octane*.

Schorlemmer's ditrityl, which boils at 123 – 125° , has the specific gravity 0.7032 at $17^{\circ}+$.

Action of nitrogen tetroxide upon the mixed octane and octylene boiling at 122 – 125° .—Many years ago Guthrie described a solid crystalline compound of amylene and nitrogen tetroxide, C₈H₁₀(NO₂)₂. So easily is this compound formed that the reaction of the gas upon amylene constitutes a very good test for the presence of the olefine, and comparatively small quantities of amylene may, by means of nitrogen tetroxide, be discovered and separated from considerable quantities of quintane.

We have studied the reaction of nitrogen tetroxide upon a mixture of octane and octylene in the hope of discovering an easier method of separating the two hydrocarbons. No solid nitro-compound, however, was

* Ann. Chem. Pharm. vol. clxi.

Ibid. p. 281.

obtained. On passing the gas into a mixture of the hydrocarbons it was rapidly absorbed, and a heavy oil separated out; this had an odour resembling that of castor-oil; but the substance could only be preserved in the freezing-mixture, and on distillation, even in steam, it was immediately decomposed.

8. The next well-defined fraction of the original liquid boiling between 100° and 200° distilled constantly at $145-148^{\circ}$. It was remarked that as this fraction increased in bulk, and the neighbouring ones became more free from octane and octylene, they gradually acquired a very characteristic and unpleasant smell, quite different from the fraction boiling at $122-125^{\circ}$. A portion of the mixed hydrocarbons was treated with bromine in the cold. The same darkening was observed as in the case of the $122-125^{\circ}$ fraction; but it was by no means so marked as in that instance, and there was no difficulty in perceiving when a decided excess of bromine had been added. The following are the details of the experiment:—

Hydrocarbon employed	18.95 grms.
Bromine needed	12.8 "
Equivalent to nonylene	10.0 "

As it was impossible to distil off the hydride, even in a current of steam, without at the same time decomposing the bromine compound, the liquid was treated with alcoholic potash and distilled upwards for some time. Water was then added, the dark brown oil separated, dried, and distilled; it commenced to boil at 150° , and fractions were collected between every 10° . The largest fractions came over at $160-170^{\circ}$ and $200-218^{\circ}$.

The portion boiling between $200-218^{\circ}$ on repeated distillation boiled almost entirely between 208° and 212° , and had approximately the composition $C_8H_{17}Br$.

0.4165 grm. bromide gave 0.3370 grm. silver bromide and 0.0105 grm. reduced silver.

	Calculated.	Found.
Br	39.0	36.3

The quantity obtained was too small to admit of purification.

A second portion of the liquid, boiling between 145° and 148° , was treated with fuming sulphuric acid. A considerable rise of temperature was observed, so that it was necessary to cool the mixture in order to prevent, as far as possible, the polymerizing action of the strong acid. The supernatant oil was then decanted, washed with dilute soda solution, dried, and distilled over sodium. About half the liquid came over below 170° , after which the thermometer rose very rapidly to above 340° , when the distillation was stopped. On cooling, the dense oily liquid in the retort solidified. The fraction boiling below 170° was redistilled over sodium; it boiled almost entirely between 145° and 150° . It was then mixed with the hydride obtained after treatment with bromine (see preceding section), and the mixed liquids were again distilled over sodium. The boiling-point

was unchanged, the greater portion of the liquid distilling at 147–148°. This liquid is nonane, as the following analysis shows:—

0.1970 grm. hydride gave 0.6080 grm. carbon dioxide and 0.2820 grm. water.

	Calculated.	Found.
C ₉	84.37	84.16
H ₂₀	15.63	15.92
	<hr/> 100.00	<hr/> 100.08

A determination of its vapour-density afforded the following data:—

Balloon with air	13.8050 grms.
Temperature of air	13.7° C.
Balloon with vapour	14.0525 grms.
Temperature on sealing	209°
Residual air	0
Capacity of balloon	116.8 cub. centims.

Vapour-density calculated for C ₉ H ₂₀ (H=1).	Found.
64.0	66.2

Its specific gravity was 0.7279 at 13° 5, compared with water at the same temperature.

It should be remarked that no liquid boiling at about 145–148° was discovered by Pelouze and Cahours in American petroleum. The nonyl hydride isolated by these chemists had the boiling-point 136–138°. We have assured ourselves that there is no liquid boiling at about this temperature in the products obtained from solid paraffin.

The boiling-point of the nonane isolated by us agrees perfectly with that calculated on the assumption that the differences in the boiling-points of the simpler members of the C_n H_{2n+2} become continually less by 4°, until the well-known difference of 19° is obtained. (Schorlemmer.)

Calculated boiling-point of C₉ H₂₀ 146° Found 147–148°

Action of chlorine upon nonane.—About 15–20 grms. of the purified hydrocarbon were heated in a small flask fitted to an upright condenser, and a current of well-dried chlorine was sent through the liquid. The apparatus was placed in direct sunlight. The action appeared to commence immediately, the liquid darkened slightly in colour, and a large quantity of hydrochloric acid was evolved. The chlorine on passing into the heated liquid appeared to burn, a flash of light appearing at the end of the delivery-tube as each successive bubble of gas passed into the hydrocarbon. In order to prevent as far as possible the formation of high chlorinated products, the passage of the chlorine was occasionally interrupted and the liquid distilled; the portion boiling below 200° was set aside, and that which came over below this point was submitted to the

further action of the chlorine. Ultimately the greater portion of the liquid was found to boil between 200° and 230° , about $\frac{1}{2}$ boiling above this point. By repeated distillation the former portion was divided into two portions—one boiling at $190-198^{\circ}$, and the other boiling constantly at $240-245^{\circ}$. The first fraction on analysis gave numbers agreeing with the composition of nonyl chloride.

I. 0.6670 grm. chloride gave 0.5937 grm. silver chloride and 0.0100 grm. metallic silver.

II. 0.5195 grm. chloride gave 0.4930 grm. silver chloride and 0.0080 grm. metallic silver.

	Calculated.	Found.	
		I.	II.
Cl	21.9	22.5	22.8

The specific gravity of the nonyl chloride was found to be 0.8962 at 14° . Pelouze and Cahours found the so-called pelargyle chloride to boil at 196° ; its specific gravity was 0.899 at 16° . It is highly probable, however, in spite of the concordance of these results, that the liquid obtained by us was a mixture of primary and secondary chlorides; and the same remark is probably applicable to the product obtained by Pelouze and Cahours. The fraction boiling at about 245° was in all probability $C_9H_{19}Cl$, produced by the further action of the chlorine upon the hydride; the quantity, however, was insufficient to purify it for analysis.

9. The next well-defined fraction distilling at a constant temperature boiled at $170-172^{\circ}$. It was treated with bromine, with the following results:—

Weight of liquid taken	4.80 grms.
„ Bromine needed	3.38 „
Equivalent to decylene	2.95 „

The liquid was too strongly reddened by the bromine; it was evident that too great an excess had been added. This would make the amount of olefine too large. The quantity of hydrocarbon taken was too small; the experiment should have been repeated on a larger scale. Our object, however, was to economize our material, as we could find no ready method of separating the brominated compound from the unattacked hydride without decomposition. Boiling with alcoholic potash gave a very indefinite result.

About 37 grms. of the mixed hydrocarbons were cautiously treated with nitric and sulphuric acids, and the mixture cooled to moderate the reaction. After standing about 17 hours the clear amber-coloured oil was separated, repeatedly washed, and distilled in a current of steam. When about $\frac{1}{2}$ of the liquid had passed over, the remainder in the flask suddenly decomposed, becoming almost solid, and fumes of oxides of nitrogen were abundantly disengaged. The distillate, which floated on water, was dried and distilled; it commenced to boil at 169° , and all came over below 175° ,

after repeated distillation over sodium, until the metal was no longer attacked; it boiled constantly at 166–168°.

It was analyzed as under:—

0.2090 grm. hydride gave 0.6460 grm. carbon dioxide and 0.2950 grm. water.

	Calculated.	Found.
C ₁₀	84.45	84.30
H ₂₂	15.55	15.69
	<hr/> 100.00	<hr/> 99.99

Its specific gravity was 0.7394 at 13°.5, compared with water at the same temperature. Schorlemmer's amyl (decatyl hydride), obtained by the action of sodium on amyl iodide, boiled at 158–159°, and had the specific gravity 0.7275 at 14°. It is very probable that this hydrocarbon is not the normal hydride, but stands to the decatyl hydride boiling at 168° in the same relation that the amyl hydride boiling at 30° does to the isomer boiling at 39°, or as the ethyl amyl boiling at 90° (sp. gr. 0.6819 at 18°.5) does to the normal heptane boiling at 99° (sp. gr. 0.6915 at 18°). The normal decatyl hydride obtained by Pelouze and Cahours from petroleum had the specific gravity 0.735 at 15°; the hydride found by Greville Williams in the oil from boghead coal had the specific gravity 0.7365 at 18°.

A determination of the vapour-density of the decatyl hydride from paraffin gave the following data:—

Balloon with air	13.9745 grms.
Temperature of air	12°
Balloon with vapour	14.2215 grms.
Temperature on sealing	222°
Residual air	0.2 cub. centims.
Capacity of balloon	105.5 cub. centims.

Vapour-density calculated for C ₁₀ H ₂₂ (H=1).	Found.
71	72.8

10. The next fraction of the liquid boiling below 200° distilled constantly between 192° and 197°, the greater portion of the liquid coming over at about 193–195°. The fraction boiling at 193–195° was treated with bromine, with the following results:—

(1) 7.63 grms. required	4.76 grms. bromine.
(2) 7.18 "	4.09 "
Mean.. 7.405	4.425

The fraction boiling at 195–197° was also treated.

7.04 grms. hydrocarbon required 3.85 grms. bromine.

Assuming that the fractions are mixtures of undecane and undecylene,

4.425 grms. of bromine would be equivalent to 3.78 grms. of the olefine, leaving 3.625 grms. of the hydride, or the fraction is a mixture of undecane and undecylene in equal proportions.

A determination of the vapour-density of the 193-195° fraction gave the following data:—

Balloon with air	13.2640 grms.
Temperature of air	16°
Balloon with vapour	13.5470 grms.
Temperature on sealing	261°
Residual air	0.8 cub. centims.
Capacity of balloon	113 cub. centims.
Found....	81.1
Calculated....	77.5

11. *Fraction boiling between 200° and 300°*.—By long-continued distillation over sodium the $2\frac{1}{2}$ litres boiling originally between these limits were resolved into the following fractions:—

- (1) 212-215°*
- (2) 230-235° solidifies at $-14^{\circ}.5\uparrow$
- (3) 252-255° ,, 5°.5
- (4) 273-276° ,, +2°.0
- (5) 290-295°

The portion boiling at 300° (in the neighbourhood of 315°) is solid at ordinary temperatures.

The following details of the bromination experiments show that the fractions were invariably mixtures of olefines and hydrides:—

Fraction 212-215°:

4.48 grms. required 2.65 grms. of bromine.

Fraction 230-235°:

4.23 grms. required 1.92 gm. of bromine.

Fraction 252-255°:

- (1) 9.38 grms. required 2.95 grms. of bromine.
- (2) 8.605 grms. required 2.67 grms. of bromine.
- (3) 5.13 grms. required 1.54 gm. of bromine.

Fraction 273-276°:

7.39 grms. hydrocarbon required 2.04 grms. of bromine.

12. It has already been mentioned that, on the first distillation of the oils obtained by splitting up the $3\frac{1}{2}$ kilogs. of paraffin, a considerable amount of substance (which could not be distilled within the range of the thermometer) remained in the retort and solidified on cooling. This substance in its crude condition melted at 31°, but on recrystallization from ether the melting-point became higher. The whole of the substance was therefore cut up into small fragments, and spread upon filter-paper

* These boiling-points are uncorrected.

† These points of solidification are only approximative.

to remove adhering oil as far as possible, and repeatedly recrystallized from ether until the melting-point was unaltered.

It melted invariably at $41^{\circ}5$ and solidified at $40^{\circ}5$; it could not be distilled within the range of the mercurial thermometer.

It crystallized from ether in beautifully white satiny scales, and gave the following numbers when burnt with oxide of copper in a stream of oxygen:—

0.3035 grm. gave 0.9590 grm. carbon dioxide and 0.419 grm. water.

Carbon	85.19
Hydrogen	15.34
	<hr/>
	100.53

Action of bromine upon the solid paraffin melting at $41^{\circ}5$.—A few grams of the substance were dissolved in bisulphide of carbon, and four minute drops of bromine added; this small quantity of bromine sufficed to colour the liquid strongly. On standing from 12 to 15 hours, the colour appeared unaltered, and the solution was not decolorized after heating from 60 – 70° for 12 hours. On heating the liquid to 150° , the colour disappeared after a few hours; and on opening the tube hydrobromic acid was evolved. About 10 drops of bromine were again added, and the liquid reheated to 150° ; but no diminution in the red colour of the solution was perceptible, although on opening the tube hydrobromic acid fumes were abundantly disengaged. This observation would appear to indicate that the hydrocarbon melting at $41^{\circ}5$ is a member of the $C_n H_{2n+2}$ series.

Action of heat upon $41^{\circ}5$ paraffin.—Repeated crystallization from ether having failed to alter the melting-point of this substance, and the behaviour with bromine evidently indicating that it belonged to the marsh-gas series, we considered that it would be interesting to study the combined action of heat and pressure upon it. The details of the experiment have been already given in section 1. After six distillations it was rendered completely liquid. It began to boil at about 40° , and about one sixth came over below 100° , about one third between 100° and 200° , and the greater portion of the remainder below 300° ; the small quantity remaining in the retort solidified on cooling.

In a second experiment the action of bromine upon the liquid hydrocarbons was quantitatively determined:

Weight of liquid hydrocarbon taken	5.73 grms.
„ bromine required before the liquid was reddened	30.7 „

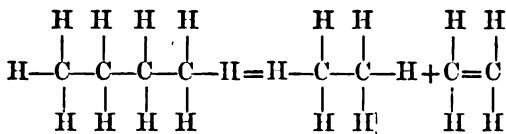
The action of the bromine was very energetic, and it was necessary to moderate the reaction by cooling the liquid.

It is evident from these experiments that the solid paraffin ($\approx C_n H_{2n+2}$) had been split up by heat and pressure into a mixture of hydrides and olefines.

13. This mode of decomposition would appear to be general, at least for the higher terms of the series of normal paraffins. If these bodies be represented constitutionally by linking the carbon atoms together in a single chain,



then the simultaneous formation of hydride and olefine must be assumed to occur from the loosening of the affinities of certain of the groups of CH_2 . Under the influence of heat these groups become dissociated (vibrate without the spheres of their mutual attractions), and recombine to form saturated hydrocarbons. Assuming (for the sake of simplicity) that this decomposition can occur so low down in the series as in the case of butane, it might be thus represented:—



We can offer but little direct evidence as to the exact manner of this decomposition—whether it is attended by the gradual elimination of ethylene, a hydride containing a greater number of carbon atoms being left behind, or whether the paraffin is at once split up into the hydride and an olefine containing an equal number of carbon atoms as in the above equation. Neither supposition is exactly substantiated by experiment. If the action of heat gave rise to the former mode of decomposition, we ought to obtain a large quantity of ethylene after prolonged heating, especially when the liquid portion is rich in hydrocarbons of low molecular weight; but, as we have already pointed out, the process of liquefaction is accompanied with the production of comparatively little gas. On the other hand, an examination of the amounts of bromine required to render the hydrocarbons boiling below 200° permanently red, shows that the proportion of hydride to olefine in the several mixtures becomes gradually larger as the molecular weight increases. It would appear, therefore, that the liquids boiling between 200° and 300° are not mixtures of hydrides and olefines in equal proportions, as are the fractions boiling at $65\text{--}70^\circ$ and $94\text{--}97^\circ$ &c.

It would have been doubtless interesting to have determined the relative amounts of the 12 fractions isolated from the decomposed paraffin; but when it is considered that their separation was only effected after several thousand distillations, it will be evident that the quantities obtained after such tedious treatment can afford no real indication of the amount present in the original liquid. We have, however, a distinct impression that the amounts of liquid boiling at $94\text{--}97^\circ$ and $122\text{--}125^\circ$ were but slightly (if at all) less than the quantities boiling at $252\text{--}255^\circ$ and $273\text{--}276^\circ$.

OBITUARY NOTICES OF FELLOWS DECEASED.

WILLIAM JOHN MACQUORN RANKINE was born at Edinburgh on the 5th July, 1820. He was the son of David Rankine (a lieutenant in the Rifle Brigade, and a younger son of Macquorn Rankine, of Drumdow, of a well-known family in the county of Ayr), and of Barbara Grahame, one of the daughters of Archibald Grahame, of Dalmarnock, a banker in Glasgow. He was educated partly at Ayr Academy, partly at the High School of Glasgow, from which he went to the University of Edinburgh; but he derived much of his instruction from his father, and, like most men who have made any real mark in science, he owed the greater part of his knowledge to his own energy and industry. In 1836 he received a gold medal for an essay on the Undulatory Theory of Light, and in 1838 he gained an extra prize for his essay on Methods of Physical Investigation. Shortly after this date he entered upon the profession of Civil Engineering, as a pupil of Sir John McNeill, under whose direction he was employed from 1839 to 1841 in various schemes for waterworks and harbour-works in the north of Ireland, and on the Dublin and Drogheda Railway. At this time he invented a method of setting out curves which still bears his name. From 1844 to 1848 he was employed, under Locke and Errington, on the construction of the Clydesdale Junction Railway, and subsequently upon various schemes promoted by the Caledonian Railway. In 1845-46 he was engineer of the proposed Edinburgh and Leith waterworks, a scheme which was defeated by the rival Edinburgh Water Company. In 1852, he and the late John Thompson were joint engineers of the well-known works by which Glasgow is supplied with pure water from Loch Katrine. In November of 1855 he was appointed Regius Professor of Civil Engineering and Mechanics in the University of Glasgow: he retained this chair until his death, which occurred on the 24th December, 1872.

It is difficult to determine whether Rankine takes his highest place as an original investigator, or in respect of his great success in digesting the scientific knowledge of himself and his contemporaries into a form available for common use. His works have the very rare merit of being *thorough*, both in a scientific and in a practical sense. With great originality of treatment, and frequently of research, his text-books exhaust the useful theory of his subjects, and his results are always reduced to a form in which they can be actually used. His practice as an engineer had made him fully alive to the important difference between the crude results of theoretical reasoning from principles and the reduced formulæ adapted to the data obtainable from observation or specification. There is probably no scientific writer to be compared with him in the three aspects of the extent covered by his treatises—their scientific accuracy and exhaustiveness and their immediate adaptation to the use of practical men, who may be utterly unable to follow the reasoning by which he arrived at his formulæ. There are persons who think that the path-finder in science is of a higher and rarer order of

intelligence than the road-maker. There are those who think otherwise ; who consider that the latter frequently displays the broader and more masculine intellect. However this may be, all rational men agree in awarding the highest meed of praise to those who can both find the path and make the road,—who can say to the little children of science, so far can you follow me—and to the maturer minds, from that point you may extend my paths. Of these men was Rankine.

Rankine's most important contributions to science are in the dynamical theory of heat, in the theory of the steam-engine, in that of waves in liquids, especially of sea-waves, and on the resistance and rolling of ships. He was among the earliest students in this country who were able to understand what was meant by thermodynamics. He contributed largely to the extension and settlement of the theory, and, probably to a greater extent than any other person, to its reduction to rules adapted to the practice of engineers ; indeed he may be considered one of the founders of the science. In the study of waves in liquids, he and Mr. William Froude appear to have been the first persons who successfully worked out a possible theory of waves of finite displacement in the sea : all the previous researches were either incomplete in theory or limited to infinitesimal disturbance. Rankine and Froude, working independently of one another, appear to have been the first to arrive at a definite demonstration of the mechanical possibility of the trochoidal wave. They were not the first to suggest that the trochoid was the clue to the geometry of the sea-wave : Gerstner and Scott Russell had already done that ; but it was Froude, in the 'Transactions of the Institution of Naval Architects,' and Rankine, in the 'Transactions of the Royal Society,' who first gave proof that it complied with all the conditions of fluid motion, except that of the absence of molecular rotation.

His study of the resistance of ships appears to have been suggested to him by an application addressed to him and to Professor (now Sir William) Thomson by his friend James Robert Napier, for advice as to the power necessary to propel vessels of any form. Professor Thomson then called attention to the defect in existing theories, with reference to the resistance of water to a ship's progress, from not taking account of the viscosity of the fluid. Professor Rankine stated that if the resistance outside a ship was the same as that inside a water-pipe, the power required to propel a certain vessel would be so and so, naming a power of about two thirds what Mr. Napier had estimated to be necessary. Alluding to this, Professor Rankine wrote, in August 1858, to the *Philosophical Magazine* (ser. 4, vol. xvi. p. 238) :—" In the course of last year there were communicated to me *in confidence* the results of a great body of experiments on the engine-power required to propel steamships of various sizes and figures at various speeds. From those results I deduced a general formula for the resistance of ships having such figures as usually occur in steamers, which, on the 23rd of December,

1857, I communicated to the owner of the experimental data; and he has since applied it to practice with complete success." This circumstance no doubt led to his addressing himself to the theory of ships. With this object in view, he investigated the form and mechanical work both of sea-waves and of waves in canals, the waves which accompany ships, the loss of work consequent on the formation of divergent waves, the stream-lines or lines of motion of water flowing past a ship, the effect of the combined oscillation of a ship and a wave, the steadying effect of keel-resistance, and many other important points of the hydraulics of a ship. On nearly all these points he did important original work, although in many of them the credit must be shared between him and others who were working abreast of him—notably Sir W. Thomson and Mr. W. Froude. He also studied the question of propulsion, and placed its theory on a sound basis. This branch of knowledge owes more to Rankine than to any other writer.

His principal published works are his 'Manual of Applied Mechanics,' a volume of about 640 pages, published in 1858; his 'Manual of the Steam-Engine and other Prime Movers,' a volume of 580 pages, published in 1859; his 'Manual of Civil Engineering,' a volume of 780 pages, published in 1862; his 'Manual of Machinery and Millwork,' a volume of 580 pages, published in 1869; and his book of 'Useful Rules and Tables,' published in 1866. Besides these, he was the corresponding editor of, and principal contributor to, the great work of 'Ship-building, Theoretical and Practical,' by Watts, Napier, Barnes, and Rankine, published in 1864-66. All these treatises have the great merits of being exhaustive, dropping out or avoiding no part of their subject, of bringing down their scientific information close upon the very date of their publication, and of having all their results reduced to a workable form. When it is added that they are terse and concise almost to a fault, and that they cover the whole of the ground from simple addition to the application of elliptic functions in pure mathematics, and from the common lever to the ellipsoids of stress and of rotation, and to the friction of gases, in applied mathematics, some idea may perhaps be formed of the immense mass of knowledge which Rankine succeeded not only in acquiring, but in reducing to a shape available in the every-day work of practical men.

As an instance of the wide generality of his interest in scientific research, may be mentioned his papers in the 'Engineer' of 1869, in which he explained the curious dynamical theory of the bicycle, or two-wheeled velocipede, which had just then come into general use. These papers are still the most complete treatise on the subject.

The joint report of Professor Rankine and Dr. Stevenson Macadam on the accident which took place in 1872 at the Tradeston Flour-Mills, by the explosive firing of air charged with flour-dust, is also a most remarkable proof of his power to handle work of a miscellaneous character.

In the 'Catalogue of Scientific Papers,' compiled under the care of the

Royal Society, Rankine is credited with eighty papers down to the year 1865. He has published a very considerable number since that date. His last, and one of his most important contributions to the Philosophical Transactions is that "On the Mathematical Theory of Stream-lines," read before the Royal Society on the 10th February, 1870. This paper contains a remarkably complete investigation of the lines of motion of particles of liquid in flowing past certain solid bodies, such as ships, and of the mechanical as well as the geometrical character of the disturbance.

It is satisfactory to reflect that his labours met with full recognition during his lifetime from the scientific bodies best qualified to judge of their merits. In 1849 he was elected a Fellow of the Royal Society of Edinburgh. In 1850 he acted as Secretary to Section A of the British Association, which then met at Edinburgh. In 1845 he was elected a Member of the Philosophical Society of Glasgow, and in 1853 a Fellow of the Royal Society of London. In 1854 he was awarded the Keith Medal of the Royal Society of Edinburgh for researches in Thermodynamics. In 1855 he was appointed one of the Visitors of Edinburgh Observatory, and in the same year Regius Professor of Civil Engineering and Mechanics in the University of Glasgow. He was also President of Section G of the British Association when it met in Glasgow in 1855. In 1856 he received the honorary degree of LL.D. of Trinity College, Dublin. In 1863 he was awarded the gold medal of the Institution of Engineers in Scotland for a paper "On the Liquefaction of Steam;" and in the same year he was President of Section A of the British Association at Newcastle-on-Tyne. In 1867 he was again President of Section G at Dundee. In December 1870 he was appointed a member of the Admiralty Committee on Ships of War, which arose out of the loss of H.M.S. 'Captain,' and he contributed largely to the scientific part of the report.

His great industry and success in the field of science were never allowed to interfere with the ordinary duties owing to society, or with those sacrifices of time which all men are called upon to make for the gratification of others. Thoroughly genial in company, affectionate and beloved in his family circle, careful and yet liberal in the relations of business, amiable and even in temper, it was as great a pleasure as it was an honour to be counted among his friends. He has not unfrequently sung a song of his own writing to music of his own composing; and, what is more, the voice, the music, and the words were all worth listening to.

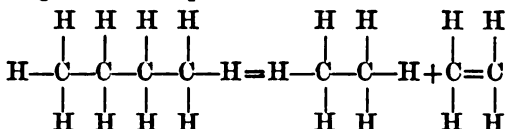
His illness began by a failing of the eyesight, which was at first supposed to be local, but which afterwards proved to be but a symptom of more deeply seated disease. Under treatment, he recovered from a severe attack of illness in October 1872, and up to within five days of his death no unfavourable symptoms occurred: then a change took place; he rapidly lost the power of speech, and the sensibility of the right side. He died on the 24th December, 1872.

He was never married. His only brother died while he was yet young, and he lost his father and his mother a year or two before his own death.

13. This mode of decomposition would appear to be general, at least for the higher terms of the series of normal paraffins. If these bodies be represented constitutionally by linking the carbon atoms together in a single chain,



then the simultaneous formation of hydride and olefine must be assumed to occur from the loosening of the affinities of certain of the groups of CH_2 . Under the influence of heat these groups become dissociated (vibrate without the spheres of their mutual attractions), and recombine to form saturated hydrocarbons. Assuming (for the sake of simplicity) that this decomposition can occur so low down in the series as in the case of butane, it might be thus represented :—



We can offer but little direct evidence as to the exact manner of this decomposition—whether it is attended by the gradual elimination of ethylene, a hydride containing a greater number of carbon atoms being left behind, or whether the paraffin is at once split up into the hydride and an olefine containing an equal number of carbon atoms as in the above equation. Neither supposition is exactly substantiated by experiment. If the action of heat gave rise to the former mode of decomposition, we ought to obtain a large quantity of ethylene after prolonged heating, especially when the liquid portion is rich in hydrocarbons of low molecular weight; but, as we have already pointed out, the process of liquefaction is accompanied with the production of comparatively little gas. On the other hand, an examination of the amounts of bromine required to render the hydrocarbons boiling below 200° permanently red, shows that the proportion of hydride to olefine in the several mixtures becomes gradually larger as the molecular weight increases. It would appear, therefore, that the liquids boiling between 200° and 300° are not mixtures of hydrides and olefines in equal proportions, as are the fractions boiling at $65-70^\circ$ and $94-97^\circ$ &c.

It would have been doubtless interesting to have determined the relative amounts of the 12 fractions isolated from the decomposed paraffin; but when it is considered that their separation was only effected after several thousand distillations, it will be evident that the quantities obtained after such tedious treatment can afford no real indication of the amount present in the original liquid. We have, however, a distinct impression that the amounts of liquid boiling at $94-97^\circ$ and $122-125^\circ$ were but slightly (if at all) less than the quantities boiling at $252-255^\circ$ and $273-276^\circ$.

March 6, 1873.

Sir GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows :—

William Aitken, M.D.	Edmund James Mills, D.Sc.
Sir Alexander Armstrong, K.C.B., M.D.	Robert Stirling Newall, F.R.A.S.
Robert Stawell Ball, LL.D.	George Edward Paget, M.D., D.C.L.
Rev. Alfred Barry, D.D., D.C.L.	Francis Polkinghorne Pascoe, F.L.S.
Edward Middleton Barry, R.A.	Oliver Pemberton, M.R.C.S.
John Beddoe, B.A., M.D.	Rev. Stephen Joseph Perry.
Isaac Lowthian Bell, F.C.S.	John Arthur Phillips, F.G.S.
George Bishop, F.R.A.S.	William Overend Priestley, M.D.
Frederick Joseph Bramwell, C.E.	Charles Bland Radcliffe, M.D.
Walter Lawry Buller, Sc.D.	Alexander Rattray, M.D., R.N.
Edwin Kilwick Calver, Staff-Capt. R.N.	Edward James Reed, C.B.
Alexander Carte, M.A., M.D.	William Chandler Roberts, F.C.S.
William Chimmo, Commander R.N.	George West Royston-Pigott, M.A., M.D.
Herbert Davies, M.D.	William Westcott Rundell.
Henry Dircks.	Osbert Salvin, M.A.
Robert Lewis John Ellery, F.R.A.S.	Major-General Henry Young Dar- racott Scott, R.E., C.B.
Joseph Fayrer, M.D.	John Spiller, F.C.S.
Peter Le Neve Foster, M.A.	The Hon. John William Strutt.
Thomas Minchin Goodeve, M.A.	George James Symons, F.M.S.
Lewis Dunbar Brodie Gordon, C.E.	Sir Henry Thompson, F.R.C.S.
James Augustus Grant, Lieut.-Col., C.B.	Edwin T. Truman, M.R.C.S.
John Eliot Howard.	Francis Henry Wenham, F.R.M.S.
Rev. A. Hume, LL.D.	Charles William Wilson, Capt. R.N.
Edmund C. Johnson, F.R.C.S.	Henry Woodward, F.G.S.
The Lord Lindsay, F.R.A.S.	Archibald Henry Plantagenet Stuart Wortley, Lieut.-Col.
Clements Robert Markham, C.B.	James Young, F.C.S.
William Mayes, Staff-Commander R.N.	

The following communications were read :—

I. "On the Vapour-density of Potassium."—Preliminary Notice.

By JAMES DEWAR and WILLIAM DITTMAR. Communicated by H. E. ROSCOE, F.R.S., Professor of Chemistry in Owens College, Manchester. Received January 27, 1873.

Since the elaborate experiments of Deville and Troost on the vapour-densities of substances at high temperatures, little has been added to chemical science in this field of research. Doubtless this is in great part owing to the difficulty of any *one* student manipulating the complex apparatus necessary for the execution of the experiments. But the operations are greatly increased in difficulty when we select bodies that are readily inflammable in air and attack with facility glass and porcelain at the high temperatures to which they are exposed. This is the reason why the molecular weights of a most important class of elementary bodies, viz. the *alkali metals* (although these are volatile at moderate temperatures), have remained to the present time undetermined. It was with the view of adding something to our knowledge in this department, that we recently undertook some experiments with potassium, the results of which we now beg leave to lay before the Society. The special difficulties we had to overcome are involved in the endeavour to answer the following questions:—

1. Is it possible to convert potassium into a gas of one atmosphere's pressure at any of the *constant* temperatures we can at present command?
2. Is it possible to generate *pure* potassium-vapour and to keep it from getting oxidized?
3. Supposing a definite volume of such vapour to have been procured, how can its *weight* be ascertained?

After a succession of failures, which we shall not detail, we at last succeeded in devising a workable process, which may be briefly described as follows:—

A cylindrical iron bottle of at least 200 cub. centims. capacity, of a thickness in the body ensuring sufficient rigidity at even a bright red heat, and provided with a well-ground inbent neck, pierced with a canal of about 2 millims. in diameter, is employed as a generator and receptacle of the vapour.

A mass of about 20 kilogra. of zinc contained in a plumbago crucible, which being placed in a forge-fire can be readily heated up to the boiling-point, serves as a bath.

The experiment begins by first deoxidizing the inside of the receptacle at a red heat by means of a current of dry hydrogen, which is continuously maintained until the bottle has cooled down below redness. At this stage about 200 grms. of pure mercury are introduced into the bottle, which is then inserted into the red-hot zinc, without, however, covering the upper extremity of the bottle. After $\frac{1}{4}$ of the mercury is distilled off (which is accomplished in a very short time), the neck is withdrawn,

and while the mercury-vapours are still streaming out, an iron test-tube, previously prepared with great care and charged with 4-5 grms. of potassium, is dropped into the bottle, the neck reinserted, and after the *whole* of the bottle has been immersed into the zinc, the blast of the forge is forcibly increased so as, in the shortest possible time, to bring the zinc into the state of boiling, proper arrangements being made for keeping the neck of the bottle red-hot. The potassium in a short time begins to volatilize, issuing in jets into the air and depositing caustic potash at the nozzle, which must be kept clear by means of an iron wire. As soon as the distillation of the potassium ceases, the nozzle is closed by means of a ground-in wire plug, at once immersed into a mass of mercury contained in a test-tube, and the bottle withdrawn to a proper support, on which it is allowed to cool.

After it has reached a manageable temperature, the bottle is inserted into a mass of recently boiled water, the wire plug withdrawn, and the hydrogen formed by the action of the water on the potassium pumped out, by means of a "Sprengel," into a eudiometer, to be measured.

In the experiments we have hitherto carried out, we have satisfied ourselves that the amount of mercury-vapour *not* swept out by the potassium is quite inappreciable; and as our object has been in the mean time to merely arrive at approximate results and to perfect our methods of manipulation, we have neglected the minute correction, which, on account of that small remnant of mercury, ought, strictly speaking, to have been applied to the volume of the vapour as calculated from the capacity of the bottle in the cold, the coefficient of expansion of iron, and the temperature (1040° Deville) at which the vapour was measured.

The results of our observations conclusively show that the density of potassium-vapour, as produced in the process described, cannot exceed 45 times that of hydrogen, and that therefore the molecule of potassium consists of *two atoms* (K_2).

We intend to prosecute our research in other directions, proposing to ascertain, if possible, the densities of the *iodides* of caesium, rubidium, and potassium, these being, according to Bunsen's experiments, the most volatile of the haloids of the alkali metals.

II. "On New Sources of Ethyl- and Methyl-Aniline." By JOHN SPILLER, F.C.S. Communicated by Dr. DEBUS, F.R.S. Received December 10, 1872.

In the process of manufacturing the Hofmann violet by the action of ethylic or methylic iodide upon rosaniline or one of its salts, there is always produced a considerable quantity of a dark-coloured resinous or pitch-like substance, which has received the name of "Hofmann gum." This by-product varies in amount and consistence according to the shade of violet simultaneously produced being much more abundant when the

iodide is employed in large proportion for the purpose of obtaining the bluer shades. It has hitherto received no technical application, but accumulates as a waste refuse in the Aniline Dye-Works where Dr. A. W. Hofmann's process is used.

Whether obtained by the ethylic or methylic reaction, the properties of this body may be briefly summarized as follows :—It is easily fusible in boiling water, and very nearly of the same gravity, for it sinks or swims with the slightest current, being itself all but insoluble in that liquid. In dilute acids (sulphuric and hydrochloric) it is freely soluble, giving dark-brown liquids, from which the gum may be again thrown down unchanged by neutralizing with alkali. Alcohol and benzol dissolve it freely, especially when heated to nearly the boiling temperature; and glacial acetic acid likewise holds it in solution, being precipitated, however, on dilution with water.

During the past year a great number of experiments have been made upon various descriptions of Hofmann gum, varying in their origin and quality according to the amount and nature of the iodide concerned in their production. The results naturally divide themselves into two series, according as they happen to be ethyl or methyl derivatives, but there appears to be a perfect parallel between the two cases; and as much interest attaches just now to the economical production of methyl-aniline, the work has rather extended in the direction of the *methyl* gums.

Without further preface, I may state that the object of this note is to announce the fact that these gums furnish a large quantity of methyl-aniline by destructive distillation. After the Hofmann gum has been kept fused for some time to drive off nearly the whole of the enclosed water, it is charged into an iron still, either with or without the addition of roughly powdered charcoal, and then submitted to a greater heat over a coke fire*. An oily body of nauseous odour soon commences to come over, and the distillation may be safely carried on until the product amounts to half the weight of the gum originally employed. Small quantities of water and ammonia commonly appear, together with a little permanent gas; but practically the oil and residual pitch may be said to be the sole resultants of the operation. This oily body is methyl-aniline, and when purified by rectification in glass retorts becomes nearly colourless, boiling at 200° C., and rising only a few degrees above that point towards the end of the distillation. Not only does it possess the peculiar odour so characteristic of methyl-aniline, but the oil has the required boiling-point as stated above, and the remarkable property of forming permanently liquid compounds (not crystallizable) with any of the ordinary acids. When acted upon with arsenic acid, at or near the boiling-point of the mixture, it furnishes a violet of somewhat red shade, which may

* A mixture of pulverized iron borings, kaolin, and syrupy silicate of soda forms an excellent lute for fixing on the head of the still, as it withstands a high temperature without softening.

be employed as a dye; and the oil gives colours of various tints when treated with other oxidizing agents, according to the usual reactions of methyl-aniline. The red-violet from the arsenical melt may, after purification, be converted into the bluer shades by the Hofmann process, when a new generation of gum is again observed.

The specific gravity of the methyl-aniline oil has varied a little in different operations, but keeps within the ranges of 0.95 and 0.97. It is probable that this trifling variation may be ascribed to admixtures of dimethyl-aniline, due in part to the introduction of higher methylated products into the crude gum employed in these experiments.

By operating in a similar manner upon the *ethyl* gums the corresponding ethyl-aniline has been obtained, the boiling-point of which was higher and not so definite (205°–210° C.) as in the case of the oil already described.

By way of conclusion, and as giving further proof towards establishing the identity of the new oils with ethyl- and methyl-aniline respectively, it may be stated that they fail to give Girard's blue when heated with rosolic acid or rosaniline, but take the peculiar course of changing slightly towards violet, and then suddenly becoming decolorized,—reactions almost without parallel in the history of the tinctorial aniline derivatives.

[Specimens of the so-called Hofmann gum, of the ethyl-aniline and methyl-aniline oils, and skeins of silk and cotton dyed with the reddish (or primitive) violet made from the latter, accompanied this communication.—J. S.]

III. "On a new Genus of Amphipod Crustaceans." By **RUDOLPH VON WILLEMÖES-SUHM**, Ph.D., Naturalist to the 'Challenger' Exploring-Expedition. Communicated by Prof. **WYVILLE THOMSON**, F.R.S., Director of the Civilian Scientific Staff of the Expedition. Received February 27, 1873.

(Abstract.)

In lat. 35° 47', long. 8° 23', off Cape St. Vincent, the trawl was sent down to a depth of 1090 fathoms on the 28th of January, and brought up, among other very interesting things, a large transparent Amphipod with enormous faceted eyes. The animal, evidently hitherto unknown, will be the type of a new genus, having the following characters:—

THAUMOPS, nov. gen.

Caput oblongum, inflatum, oculis maximis superiorem capitis partem tegentibus. Segmenta thoracica 6, abdominalia 5. Antennarum in feminis par unum, maxillarum par unum, pedum paria duo minima maxillarum locum tenentia. Mandibulæ nullæ. Pedes thoracici 5, abdominales 3 in quoque latere. Appendices caudales 4, Gangliorum pectoralium paria 5, abdominalium 3.

Thaumops pellucida, n. sp.

Corpus longitudine 14 mm., latitudine 21 mm., pellucidum.

An anatomical description of this interesting animal is given, illustrated by two plates; and it is shown that, among the Amphipods known to us, *Phronima* is its nearest relation. But there are so many points in which this genus differs from *Phronima*, that it cannot form a member of the family Phronimidae; and I therefore propose to establish for it a new family, Thaumopidae, belonging to the tribe of *Hyperina*.

The form of the head is totally different from that of *Phronima*; the antennæ are not situated near the mouth, but at its front, and the enormous faceted eyes occupy its upper surface. The first two pairs of thoracic appendages are not, as in *Phronima*, ambulatory legs, but maxillipeds, so that only five pairs of legs are ambulatory in *Thaumops*. The thorax is composed of six segments—the first of which has, on its underside, the vulva and one pair of maxillipeds; and the second, representing two segments, bears two pairs of appendages, the larger maxilliped and the first pair of ambulatory legs. The abdomen consists of five segments, with three pairs of pedes spurii, the caudal appendages being attached to the fourth and fifth segments.

The animal being beautifully transparent, the nervous system could be carefully worked out without dissecting it; the position of the nerves going out from the cephalic ganglion, as well as that of the five pairs of thoracic and the three pairs of abdominal ganglia, could be ascertained. The eyes, having at their borders very peculiar appendages, were examined, and a description is given of the structure of the large crystalline bodies which are to be seen in them. Organs of hearing and touch have not been discovered.

The mouth is covered by a pair of maxillæ and a small labium. There is a recurved œsophageal passage leading into a large cæcal stomach, and an intestinal tube departing from near the end of the œsophagus and running straight to the anus.

The heart is an elongated tube extending from the second to the fifth segment, with probably three openings. Three pairs of transparent sac-like gills are attached at the base of the second, third, and fourth pairs of feet.

Genital organs.—The single specimen taken is a female. The ovary, probably composed of two ovaries, has a rose-colour, and the genital papilla is situated at the under part of the first segment; it is covered by two small lamellæ, which in this case did not sustain the eggs, which were found to be attached to the first pair of ambulatory legs. The animal seems to carry them in a manner similar to the pycnogonid *Nymphon*.

Development.—The eggs contained embryos having already the antennæ,

the five pairs of legs, and the abdominal feet; they show that *Thaumops* has to undergo no metamorphosis, and that the young ones leave the eggs with all their appendages well developed.

Mode of life.—It could not be made out whether *T. pellucida* inhabits the deep sea, or whether it is, like *Phronima*, a pelagic animal, having been caught by the trawl only as the latter came up from the depths.

H.M.S. 'Challenger,' Tenerife,
February 13, 1873.

March 13, 1873.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

The following communications were read :—

I. "Note on Supersaturated Saline Solutions." By CHARLES TOMLINSON, F.R.S. Received February 10, 1873.

In the year 1866, M. Gernez and M. Viollette published each a memoir on supersaturated saline solutions*, in which the same conclusions are arrived at, namely :—(1) that the only nucleus capable of suddenly crystallizing any one of such solutions is a salt of the same kind as that dissolved; and (2) that all bodies, solid, liquid, or æriform, which apparently act as nuclei, are really contaminated with a hydrate of the salt that forms the supersaturated solution.

I cannot refrain from expressing my admiration at the unwearied skill and patience with which these two memoirs were prepared. The experiments were repeated by hundreds, and under a large variety of circumstances, so that it seems scarcely possible to entertain any doubt as to the validity of the conclusions arrived at. I had not seen these memoirs until long after the publication of my second paper on this subject†, or I should have hesitated in offering it to the Royal Society without special reference to them. What I did see was a very brief abstract of M. Gernez's memoir in the 'Comptes Rendus,' and to this I refer in my first paper‡, quoting the experiments and the decisive objections of M. Jeannel in opposition to M. Gernez's conclusions.

The experiments of Mr. Liversidge§ are identical in principle with

* Annales Scientifiques de l'École Normale Supérieure, tome 3^e, Année 1866, pp. 167 and 205. I am indebted for this reference to the courtesy of M. l'Abbé Moigno.

† Phil. Trans. 1871, p. 51.

‡ Phil. Trans. 1868, p. 660. The reference to M. Jeannel is in the abstract of this paper contained in the 'Proceedings of the Royal Society,' May 28, 1868, p. 405.

§ Proceedings of the Royal Society, vol. xx. p. 497.

those of M. Viollette, and some of them mere variations, such as the proof that bodies greedy of water and capable of being hydrated do not produce crystallization ; only M. Viollette made use of calcined sulphate of copper instead of calcic chloride &c.* Moreover, no fresh proofs are wanted as to the non-nuclear action of the modified salt or of the anhydrous salt on supersaturated solutions of Glauber's salt.

Recently, MM. Gernez† and Viollette‡ have each, on the occasion of the publication of my third paper (namely, that written in conjunction with M. Van der Mensbrugghe§), called in question the integrity of my experiments, M. Gernez insisting that the oils and other liquids employed by me contain salts of the same kind as those of the solutions they apparently acted on.

Such an assertion as this seems to me to be very difficult of proof ; and it seems equally bold to assume that the air of an open garden in the country contains salts of various kinds, watching their opportunity to get into my flasks and vitiate the results of my experiments.

I desire to invite special attention to a statement made in my second paper ; the more so because, without desiring for a moment to call in question the negative results obtained by MM. Gernez and Viollette, namely, that pure oils &c., as used by them, whether in the form of films or globules, do not cause the solutions to crystallize, the method indicated opens a new process for ascertaining whether there is really any other nucleus except a salt of the same kind. The statement referred to is as follows :—

“A solution of two parts of Glauber's salt to one part of water was boiled and filtered into three flasks, which were covered with watch-glasses and left until the next day. A drop of castor-oil was then placed upon the surface of each : it formed a lens which gradually flattened ; but there was no separation of salt, even when the flasks were shaken so as to break up the oil into small globules. . . . If, while the flask is being turned round, a sudden jerk be given to it, so as to flatten some of the globules against the side into films, the whole solution instantly becomes solid”||.

I desire on the present occasion to describe a number of experiments in which this process has been observed. But anticipating the objection that the various oils that have been long on my shelves, and already used in former experiments, may really contain various kinds of saline nuclei, I procured from a wholesale house a number of fresh specimens of oil. These were the oils of lavender, bergamot, cajuput, sesame, rape, almonds,

* On the surface-tension theory, it is not inaccurate to say that absolute alcohol robs the solution of water, and so determines crystallization, since the mixture of alcohol and water degrades the tension of the solution.

† *Comptes Rendus*, vol. lxxv. p. 1705.

‡ *Ibid.* vol. lxxvi. p. 171.

§ *Proceedings of the Royal Society*, vol. xx. p. 342.

|| *Phil. Trans.* 1871, p. 55.

olive, and sperm. I did not allow these oils once to enter my laboratory, but kept them in my library, and never opened the phials except in the open air.

The flasks and dropping-tubes were washed in strong sulphuric acid, and rinsed in tap-water, the tubes being kept immersed in clean water in the open air. The solutions were filtered into the flasks, and these, being covered with small beakers, were reboiled until steam issued from the orifice.

The flasks were then taken into my garden, and when cold an oil was dropped upon the surface of each solution. If the oil formed a well-shaped lens, there was no separation of salt; and in many cases, instead of restoring the small beaker to its place, a well-fitting cork was driven into the neck, and the flasks were thus left for a time, on some occasions extending to the next day.

In order to avoid the objection that during the shaking crystallization might be produced by some of the solution splashing against the cork, many of these experiments were repeated in a pear-shaped flask, nearly 12 inches in height, and containing about 2 or 3 ounces of the solution. Most of the experiments were tried in globular flasks of 5 ounces capacity, with straight cylindrical necks 4 inches high.

Experiment 1. Sodic sulphate—1 salt, 1 water. Solution in tall flask covered with a small beaker. Next day oil of sweet almonds was dropped into it, and the flask corked. After about an hour a circular motion was given to the flask, so as to disperse the oil into minute globules through the solution, giving it the appearance of an emulsion. The flask was left at rest during about an hour, then suddenly shaken, so as to rattle the solution against the side, when all at once, as if with a flash, it became solid.

Now in this case it may be objected that a crystal of the sodic sulphate hydrate was derived from one of the following sources:—(1) from the air, (2) from the oil, (3) from the cork, or (4) from the side of the flask. (1) It could not be derived from the air, either of the laboratory or of the garden, because the solution remained liquid long after the flask had been corked. (2) It could not have been derived from the oil, because this was dispersed through the solution in myriads of globules, without any nuclear action, and the flask was left to repose for an hour after the oil had been so dispersed. (3) Nor could any nucleus have been derived from the cork, because the solution never touched it. Nor could a minute speck of the sodic sulphate hydrate have fallen from the cork; for the latter had been put into hot water, out of which it was taken the moment it was put into the flask. It could not have been derived from any of the hot water from the cork streaming down the side of the flask to the solution, because sodic sulphate in solution is in the non-nuclear anhydrous state, and also because two hours had elapsed between the corking of the flask and the solidification of the solution. (4) A crystal could

not have been derived from the walls of the flask, because the solution had been briskly boiled in it, so that steam escaped with considerable force from the neck after the small beaker had been put on. Hence I am compelled to fall back on my former statement*—namely, that the oil acted as a nucleus by the flattening of one or more of the globules against the wall of the flask into the form of film.

A globular flask, containing the same solution and the same oil, was corked, and the oil dispersed in globules. It was left some hours, with occasional shaking, so as to rattle the solution against the side. The rattling motion was purposely less energetic than in the former case, to avoid splashing against the cork. This solution crystallized after it had been left a short time at rest; and it did so in large flat crystals, described in my second paper, very different from the minute radial action noticed when crystallization sets in from a point†.

A similar flask, containing the same solution, was treated with oil of bergamot, a drop of which was allowed to trickle down the side of the flask. As soon as the oil touched the solution, crystallization set in, the radiant-point coinciding with such point of contact, and the whole surface was covered with fine lines diverging from this point alone.

In another case a drop of the oil of rosemary was deposited on the centre of the solution. On shaking the flask, the solution assumed the solid state.

Two flasks, containing the same solution, crystallized under the influence of oil of lavender.

Five flasks, with the same solution, crystallized under the action of sperm-oil.

Experiment 2. Sodic sulphate—2 salt, 1 water. In three flasks the solution crystallized on the addition of olive-oil, the first immediately, and the other two after being shaken.

Experiment 3. Sodic sulphate—3 salt, 1 water. This solution crystallized under the action of the oils of bergamot, almonds, colza, rape, and sesame.

Experiment 4. Sodic sulphate—3 salt, $1\frac{1}{2}$ water, in six flasks, all of which crystallized under the action of sesame oil.

Experiment 5. Potash alum—8 salt, 3 water. The solution crystallized suddenly, as with a flash, on being shaken up with oil of olives. This is the more remarkable, as the usual action of a nucleus is to produce a solitary octahedron, which grows rapidly until the solution becomes solid. This last effect was produced by a less violent shaking of the solution in contact with oil of lavender.

Experiment 6. Ammonia alum—8 salt, 7 water. On shaking the solution with olive-oil it suddenly became solid, and of an opaque chalky white.

This selection from a large number of experiments may be sufficient to

* Phil. Trans. 1871, p. 52 *et seq.*

† *Ibid.* p. 54.

answer my present purpose—namely, to show that some supersaturated saline solutions really do crystallize under the action of other nuclei than a salt of the same kind as that of the solution operated on.

My experiments were performed in the open air, at temperatures between 30° and 50° F. At comparatively low temperatures the shaking of the flask containing the solution sometimes produced a copious liberation of anhydrous salt, which rapidly combines with water and forms the 7-atom hydrate*; but on allowing the flask to rest for some time, it generally happens that the solution becomes solid, and the 7-atom salt opaque white.

I may perhaps be allowed to state that my experiments, conducted as they were in the open air, were delayed by the rainy weather of last year. There is, however, this advantage in wet weather, that the saline particles said to exist in the air are washed down and brought into solution, in which condition they are not nuclear, as I have already shown in the case of sodic sulphate, alum, and one or two other salts†. On the other hand, fine weather has its advantages; not only are the surfaces of the solutions more active and the evaporative force stronger, but hydrated salts, said to exist in the air, part with their water of crystallization so readily as to reduce them to the non-nuclear condition. This is especially the case with sodic sulphate. I have already shown that supersaturated solutions of this salt may be exposed to the air, both in fine and wet weather, for a long time without crystallizing‡, as well as the fact just noticed§, that a solution of a salt does not act as a nucleus to its supersaturated solution. Moreover, if sodic sulphate exist in the dusty air of a room, it cannot retain the hydrated form during many minutes, but must rapidly pass into the anhydrous, in which it is no longer a nucleus.

* The view adopted by me that the supersaturated solution of sodic sulphate contains the anhydrous salt in solution, and that this is first thrown down on lowering the temperature, agitating, &c., has been objected to, on the ground that, according to Löwel, it is the 7-atom hydrate that is really in solution and is deposited on cooling. There are numerous proofs that it is the anhydrous salt which is really in solution; these I have collected in two papers, contained in the 'Chemical News' of 3rd and 10th December 1869. That the 7-atom hydrate is built up on the anhydrous salt may, I think, be shown by an experiment. Two flasks containing a solidified solution of sodic sulphate of the same strength are heated over a spirit-lamp; one of the flasks is constantly turned round on the ring of the retort-stand until the whole of the salt has entered into solution; it is then boiled, closed, and set aside. The other flask, during the heating, is allowed to remain at rest over the flame for a short time, so as to liberate a portion of the anhydrous salt. This flask is also boiled, although the operation is interrupted by violent bumpings. It is closed and placed by the side of the other flask, under the same conditions. When both flasks are cooled down to the temperature of the air (say about 50°), the flask containing the anhydrous salt will contain a crop of the 7-atom hydrate, built upon the anhydrous deposit. The other flask will have no crystalline deposit at all.

† *Chemical News*, February 4, 1870, p. 52.

‡ *Proceedings of the Royal Society*, 1871, p. 41.

§ *Chemical News*, February 4, 1870, p. 52.

Seeing, then, that in the case of sodic sulphate, which is said to be always present in the air of rooms, and, according to MM. Gernez and Viollette, even in that of the country, the chances are that it is most likely to be present either in the effloresced condition or in solution, and equally non-nuclear in both, I cannot help thinking that too much importance has been given to this part of the subject; for if it be true, we are reduced to the dilemma, pointed out by M. Jeannel*, that there must be floating in the air specimens of all kinds of salts that form supersaturated solutions and crystallize by the introduction of a solid nucleus; whereas there are some such salts which cannot exist in the presence of the oxygen or of the ammonia of the air.

II. "Visible Direction: being an Elementary Contribution to the Study of Monocular and Binocular Vision." By JAMES JAGO, M.D. Oxon., A.B. Cantab., F.R.S. Received February 12, 1873.

(Abstract.)

It is a well-known fact that when the eye has been displaced in its socket, as, for instance, by the tip of the finger applied to the eyeball through the eyelid, all objects seen by it deviate from their true directions; and the author's mode of proceeding in this paper is to inquire whether visual deviations that may be observed in arbitrary, but methodically devised, displacements of the eyeball in its socket follow any law, and then to consider how far the results thus derived are conformable with other monocular and binocular experiences, and how far they may be available in the explanation of certain phenomena that have been deemed anomalous in physiological optics.

Having pointed out means by which the ball may be easily displaced in any direction, he draws attention to the fact that, when by such means the apparent directions of objects seen by the eye are made to deviate from their true directions through fully 30° , the orbital muscles so fully retain their command over the movements of the eyeball, that that point in the visual field which was painted on the point of direct sight in the centre of the foramen centrale retinae still continues to be there painted. He shows this to happen whatever be the direction in which the eyeball is displaced in its orbit.

This fact being a fundamental one in the inquiry he has in hand, he puts it to nicer tests still.

He adjusts the two eyes, when equally displaced so as to cause objects to deviate greatly from their true directions, to look awhile at the top of a high object in the open air, and having obtained a strong *spectrum* of this object in the retinae, he, with the released eyes, looks at an appro-

* Ann. de Ch. et de Ph. 4th ser. vol. vi. p. 100.

priate mark on a grey wall, and finds that the spectrum really has its margin across the point of direct sight; and he tries other experiments in corroboration.

Also by agitating a pin-hole in a card across the eye when looking at such a high object, he brings the retina into view, and sees that the point of direct sight is visibly within the foramen centrale retinæ, as made visible by the shadow of the wall that bounds the foramen. He indicates other means of proving the same fact.

He gathers from a series of experiments that the mastery of the orbital muscles over such movements of the eyeball as are requisite for pointing the optic axis to its objective point, is practically unimpaired by such shiftings of the eyeball in its socket as have been described.

He then proceeds to show that the regulating duties of the orbital muscles, when the eyeball is displaced in its orbit, are not only fulfilled as to the rotation of the optic axis about a central point, but as to the rotation of the eyeball about this axis. To make experiments to this end, we must have another subjectively visible retinal spot besides the foramen centrale; and this we negatively have in the punctum cæcum, or at the base of the optic nerve.

A diagram is devised by which we may manage that one point of it shall be seen by the direct sight of both eyes, whilst another point is found to fall in the middle of the blind spot of one eye; and the diagram is examined by this eye when this has been pushed from its orbital place upwards, downwards, inwards, and outwards, and in various oblique directions, besides when more or less twisted on its axis; and thus it is demonstrated that what happened with the point of direct sight in the retina happens equally surely for every other retinal point—that under all these displacements the orbital muscles do not forfeit their control of the eyeball, but so regulate its movements that the different points of the field of vision remain constantly painted on the same retinal points.

From these and other methodically continued experiments, he draws the general inference, that if the centre of the foramen centrale retinæ be forced at any instant from its position by any sort of manipulation, and then made to describe a circle round its first position of ease whilst the optic axis has never ceased to remain parallel to its first direction (that is, has generated a cylinder in revolving), the axis of the *seeming* field of vision will have so revolved as to have generated a cone, whose apex is posterior to the retina in the first or undisturbed direction of the optic axis. The like might have been said of any other normal to the retina, the axis of the base of the optic nerve, for instance, were it accessible to light; whilst a twisting retinal movement about a fixed axis twists the *seeming* field of vision.

If the optic axis revolve so as to generate a cone whose apex is in front of the eye, the axis of the *seeming* field may, according to circumstances, generate a cylinder, or a more acute cone enclosing the other.

Conversely, the parallax of the visual field being noted, we can assign the retinal displacements that have produced them.

Should undue contraction of any orbital muscle, or discordant contractions of the orbital muscles, engender visual parallaxes, we may as safely judge from these parallaxes of the retinal displacements that must have been induced, as if they had been due to manipulation of the eyeball.

In these summarized conclusions we have the means of solving highly important problems in physiological optics.

It is found that sensation, or the function of responding to objective light, is exclusively resident in the retinal elements of the bacillar layer, but that the visual functions of the retina extend no further; for it has been evinced in manifold experiments that when the axes of all the pencils of objective light which concur in imaging a picture upon the retina are normals to its surface, any point in the picture may be *perceived* as lying in successive directions, forming very variable angles with and round about its normal. The retina cannot inform us of the visible direction of any point painted on its surface.

This being so, there is no alternative but to seek for a solution of the mystery in the structure out of which the retina proceeds, for the property in question is plainly inherent in the visual nervous apparatus.

The author recalls that he had long ago pointed out that, though the optic nerve in its orbital course and its fibres in their retinal course are obnoxious to mechanical pressure, no visual sensation can be immediately produced by such pressure on nerve-trunks or branches. Sensation can only be produced by pressure through the sclerotic by affecting the rods and cones of the bacillar layer, and then only when the flexure of the retina crowds together the *internal* (as to the eye) ends of the bacillar elements. He cites his former words:—"When we turn in the dark the eyeballs sharply, or even mildly, a couple of white circular rings, brighter at one margin than the other, enclosing a paler area with a central dark spot, flash forth, the diameter extending an angle of several degrees. The phenomenon is plainly the result of flexure of the retina where the nerve runs into it, as the eye is pulled round in its socket until it drags upon the nerve; and it is to be noted that it is *again* where the inner retinal elements are squeezed laterally that the phenomenon is disclosed." The absence of the tough and dense sclerotic where the nerve penetrates it, as well as of the choroid, indicates how readily the nerve must yield to the slightest traction.

In these previously recorded facts the author feels assured that he had, unwittingly, provided himself with a key to the secret of visible direction.

For it has been shown by diversified experiments that whenever there is a parallax in visible direction it is accompanied with a displacement of the base of the optic nerve in the same direction, that is to say, with *trac-*

tion upon the nerve-stem, tending to carry its distal extremity that way. The "white circular rings, brighter at one margin than the other," have been instanced as proclaiming that such traction cannot occur without flexure between the nerve-stem and the eye-apple, which displays itself at the junction of the optic disk with the surrounding retinal expansion. In other words, under the concordant action of the orbital muscles, all the movements of the globe are so equably coordinated that the nerve-stem is never subjected to unwonted traction, and consequently always emerges through the ocular tissues to open out into the retina as a normal to their surfaces, in which case no visual parallax appears. But no sooner is there lateral traction than the axis of the emergent nerve-stem, or of the optic disk, deviates from the said normality; and were that disk impressible by objective light, its central point would deviate in the same direction, and an equal deviation in visible direction would be associated with every other point in the visual field.

Hence we are fairly landed upon the conclusion that visible direction, which has already been tracked backwards to the optic nerve, is a function of its terminal direction, being identical with it at the centre of the optic disk, both in the equable use of the eye and in the unequable.

Finally, it is clear that if the eyeball be twisted round the axis of the optic disk the terminal portion of the nerve will be twisted in the same direction; and thus the opposite twisting of the visible field in certain experiments related are explicable by the same hypothesis—an hypothesis that accounts for all the phenomena of visible direction, whether regular or irregular.

Whenever the inverted retinal image, by means of nervous arrangement, is reinverted, an erect image is seemingly projected, if not from, by means of the base of the optic nerve.

The principles here arrived at, when applied to binocular vision, lead to the observation of phenomena that have not been before put on record.

Wheatstone, in his classic paper in the *Philosophical Transactions*, wherein he announces his discovery of the stereoscope and expounds its theory, only speaks of stereoscopic vision from two perspectives, an appropriate one for each eye, when (no instrument being used) the optic axes meet each other beyond them, or have previously intersected, so that each eye sees the other's perspective—that is, in all experiments by him and other subsequent writers on the subject the optic axes have always been supposed to intersect or to lie in one plane.

But as it has been demonstrated that the axes of visible direction need not be coincident with the optic axes, it ought to follow that we may continue to see bodies in relief from a pair of stereoscopic perspectives, though these are not placed transversely to each other.

Two perspectives of a pyramid are drawn, such as, when placed laterally apart as is usual in stereoscopic slides, and looked at by concurrence of the

optic axes beyond them, they yield a hollow pyramid, and when looked at by a previous decussation of these axes yield a solid pyramid. But these perspectives are placed so that the one which was at the left has the one that was at the right immediately underneath it, with about half an inch of plain paper between them.

Then it comes to pass that by properly displacing the right eyeball upwards, by means of the tip of the finger placed underneath it, we can put the under perspective immediately upon the upper one seen with the other eye, and thus realize the hollow pyramid; or by placing the finger upon the top of the left eye, we can depress the upper perspective to cover the under one, and thus realize the solid pyramid. The first pyramid depends from the plane of the paper, the second stands upon it.

By means of a finger under one eye and another upon the other, we can obtain either hollow or solid pyramids anywhere between the two perspectives; or by a finger on both eyes, or under both, we can obtain the pyramid and its "converse" below or above both perspective outlines.

In all these cases the optic axes do not intercept each other, but the axes of visible direction (functional of the final directions of the optic nerves) do meet on the paper—that is, the first pair of axes are not, and the second pair are, in such a realization as is herein planned, in one plane.

In all these cases the perspectives fall on similar parts of the two retinæ, as in the modes originally mentioned by Wheatstone.

The author goes on to consider in what way the sensorium refers the sensations it receives notice of from the optic nerves into space, so as to fix the place, size, and form of an object.

The theory of vision in retinal normals being proved to be untenable, it is admitted that there is some such association of the two retinæ as to have fairly suggested the theory of "identical" or "covering" points; but this relation he believes to be subordinate to a law by which the sensorium *projects* or *emits* its perceptions into space, as it were in two imaginary cones of *sight-rays*, which, though not issuing from the ends of the optic nerves, have apices whose positions are functional of the directions of these ends for the instant in question; and that it is by the intersection of the sight-rays in these cones, limited by the law of similar retinal parts, that the places, sizes, and forms of objects are determined. Hence if we conceive that a pair of stereoscopic perspectives, one being imaged on one retina and one on the other, exist as sight affections in miniature in the substance of the optic nerves, the size of the resultant solid form will be greater the greater is the distance from the nerves at which the axes of visible direction intersect, or the optic axes when they are coincident respectively with them.

The paper concludes by exemplifying in sundry ways the modes in which the conclusions in it may be applied in investigating seemingly anomalous phenomena in physiological optics.

March 20, 1873.

Mr. GEORGE BUSK, Vice-President, in the Chair.

The following communications were read :—

- I. "On the Distribution of the Invertebrata in relation to the Theory of Evolution." By JOHN D. MACDONALD, M.D., F.R.S., Staff Surgeon R.N., Assistant Professor of Naval Hygiene, Netley Medical School. Received February 26, 1873.

All organized beings exhibit both structural and functional conditions, forming the grounds of comparison by which natural affinities, in smaller groups, and points of difference in larger ones, are detected and established in systematic classification.

General anatomical or physiological considerations in agreement are usually of more importance than the harmony of single or special conditions of either description; and though structural characters, as a rule, are superior to those of a functional nature, much may be learnt from an arrangement founded on physiological principles alone. I have elsewhere pointed out the deceptiveness of taking the habit of life as a guide in classification, though this is adopted by many zoologists; for essentially different types may live under precisely similar circumstances, or the habit of life may be very different in the members of the same type. Thus, if we look upon a pectinate gill for aquatic respiration, fluviatile or marine, and the amphibious coincidence of this with a pulmonary chamber, or the presence of the latter cavity alone in purely terrestrial Gasteropods, as grouping characters, nothing can be more erroneous; for all these conditions of the respiratory system are to be met with in unequivocal examples of the same group, anatomically defined, as in the Nerite alliance, or that of *Rissoa* for example. Nevertheless animals so simple in their nature as the Protozoa may be distributed physiologically, with some show of truthfulness, in the resulting scheme.

Passing the leading types of the Protozoa in review, we notice that the Gregarinidæ alone are essentially parasitic in their habit of life, obtaining nutriment from materials elaborated by other animals. All the rest are therefore non-parasitic, deriving their sustenance from the outer world. If we now consider the manner in which nutritious matters are taken up and assimilated by these animals, we find that some of them must subsist on organic substances in solution, which are absorbed by the general surface of the body. Moreover we observe that this takes place either indirectly through a more or less consistent

investing substance, or directly through the pores, foramina, or fenestrations of the calcareous or siliceous capsules protecting the contained sarcode-bodies. In other instances, on the contrary, solid food is actually consumed by mouthless beings, which simply open their bodies to receive it; and this opening of the body may take place at any part of the surface most convenient, or it may be restricted to a definite locality, shadowing forth the permanent mouth of the Stomatoda, or even that of the most primitive form of Hydrozoa.

The annexed Table of arrangement is drawn up in accordance with the foregoing remarks.

Physiological Classification of the Protozoa.

Habit of life and mode of nutrition :—

I. Parasitic	<i>Gregarinidæ.</i>
II. Non-parasitic.	
A. Assuming food in a state of solution by absorption of the general surface.	
1. Indirectly through a medium	
a. Forming a cell-like envelope	<i>Thalassicollidæ.</i>
b. Lining porous canals in the common mass	<i>Porifera.</i>
2. Directly through	
a. The pores or foramina of a calcareous shell	<i>Foraminifera.</i>
b. Fenestrations of a siliceous shell	<i>Polycystina.</i>
c. A more largely exposed surface	<i>Acanthometridæ.</i>
B. Assuming solid food by an adventitious mouth.	
1. At any part of the surface where the contact is made ...	<i>Monera, Amæba, &c.</i>
2. At a definite part, determined by the opening of the shell	<i>Gromia, Diffugia, &c.</i>
C. Assuming solid food by a permanent mouth.	
1. The same orifice being also excretory	<i>Infusoria.</i>
2. Discharging excreta by a rudimentary anus	<i>Noctilucidæ.</i>

This Table may be said to afford us good general grounds for forming an estimate of the relative superiority of the several types thus physiologically defined, and it is mainly in keeping with their more commonly received distribution founded on structural particulars.

A show of progressive improvement is seen in the respective sections A, B, and C, though to all appearance the simplest group of animals in existence, namely the *Monera* of Hæckel, is included in the section B. These rudimentary creatures are destitute of both nucleus and contractile vesicle, though exhibiting activities in movement, taking food, and reproducing their kind, not even second to those of *Amæba* and its allies. The smallest ciliated molecule endowed with animal life could not present a more simple structure than that of the perfectly homogeneous and jelly-like *Monera*. Indeed the evolution of any of the other primitive forms from a plastic source like this is quite conceivable, though of course we have no actual means of observing such a transmutation.

Moreover the development of amœboids in some part of the life-history of most Protozoa would appear to stamp that form as the

earliest genetic type of beings. With the exception of a nucleus and a contractile vesicle, *Amœba* itself may have sprung from *Protamœba*; and the finally encysted jelly-globules of *Protomyxa* and *Myxastrum* breaking up into naked amœboids, or pseudonavicellæ liberating them, very strikingly suggest the source from which the *Gregarinæ* may have been evolved.

The valuable researches of Mr. Archer, of Dublin, have brought to light many very interesting freshwater Protozoa, thus much augmenting our materials for comparison, and adding new zest to inquiry as to their natural affinities or their probable origin and derivatives.

If evolutionary forces are admitted to be in constant operation, it would be hard to say that any two existing forms should stand to each other in the relation of source and product. It would perhaps be safer to say that existing forms have taken their origin from *such* forms as are still in existence; for as it is but reasonable to suppose that in the lapse of time all the members of the primary type must have undergone some change, the persistence of that type through all in its primitive state is difficult to conceive, though, for any thing we yet know, this may be the case.

Without indulging in this theme further, if we now seek for the most probable derivatives of definite types of Protozoa, some remarkable facts strike us first in relation to the cestoid worms as bearing upon their possible derivation from the Gregarinidæ. I have already noticed the affinity of the Gregarinians themselves to *Protomyxa* and *Myxastrum* amongst the *Monera*; but when we find the hooklets of *Tænia* and the sucker-pits of *Tænia* and *Bothryocephalus* shadowed forth in *Hoplo-rhynchus* and *Actinocephalus* respectively, we can scarcely help acknowledging the alliance here indicated. In the Gregarinidæ, moreover, there is not only a distinct external integument, but Van Beneden has lately demonstrated the existence of circular muscular fibres on its inner surface; a similar habit of life in both cases is also very significant. Nor would it be inconsistent to regard the Trematoda and Nematoidea as further developments of the same series of essentially internal parasites.

Now, although the Thalassicollidæ are not parasites, the genus *Thalassicolla* and the *Gregarinæ* alone of all the simple Protozoa take up their nutriment in solution, after the manner of the compound forms, namely the Porifera, restricted Polycystina, and Foraminifera. This fact, I think, is significant, as suggesting the derivation of *Gregarina* from some such original as *Thalassicolla*, as it does not seem natural to suppose that the former, which is so essentially an Entozoon, could have been descended from a stock capable of assuming solid food in the outer world.

Dr. Carpenter unconsciously gives us the weight of his opinion in the following quotations from his valuable work on the microscope. On page 449 he says, speaking of *Sphærozoum*, "Towards the inner surface of this (the outer) coat are scattered a great number of oval bodies resembling

cells, having a tolerably distinct membraniform wall and a conspicuous round central nucleus, thus corresponding closely with the *Gregarina* type." I might mention in passing that, having frequently taken in the towing-net the unequivocal allies of *Dictyocha* with sarcode bodies identical with those of *Sphærozoum*, I have no hesitation in assuming *Dictyocha* itself to belong rather to the Thalassicollidæ than to the group with which it is more usually associated. This family is commonly included under the head of Rhizopoda; and there can be no doubt that the generalization, irrespective of that term, is a correct one; but it is a stretch of transcendental anatomy to speak of the existence of pseudopodia in any member of it. The radiating branched filaments within the dense external investment of *Thalassicolla nucleata* are not extensions of the sarcode-body, like those of *Gromia* for example, but apparently act as retinacula, and as conduits for dialytic currents, which may account for the phenomenon of cyclosis observed in some instances.

Professor James-Clark, of Pennsylvania, appears to have satisfied himself, at least, that there is a remarkable agreement of characters exhibited between the Porifera and the Infusoria, which are connected, as he endeavours to show, by a regular gradation of animals. The derivation of the latter group of Protozoa from the former, which I had myself assumed quite independently, is therefore supported by that gentleman's researches.

Even with our present advanced knowledge of the Infusoria it is doubtful if we do not still include amongst them the larvæ of *Turbellaria*; and, indeed, the passage from the one type to the other would appear to be natural and easy. On the other hand, tracing through such forms as *Nemertes*, *Bonellia*, and *Priapulius*, *Sipunculus* will lead directly to the less equivocal Echinodermata; and here the series must wind up, for further evolution, though perhaps possible, does not appear to have taken place.

The existence of such low or simple forms of Rotifera as the genus *Asplanchna*, for example, would be favourable to the idea that the Noctilucidæ might have been the progenitors of that order of beings. It is of course quite gratuitous, but convenient, at present to assume that the Noctilucidæ would thus hold the same relationship to the Polycystina that the Infusoria appear to do to the Porifera. However this may be, it is more certain that the Rotifera are at the root of the annulose and articulate series.

From the Rotifera, through the Annelida, we may thus trace the development of the crustaceous and chitinous types of Articulata like a dichotomous branch.

The Annelida may be linked with the Crustacea by means of the Sagittidæ, whose exquisitely striped muscular fibres accord to them a higher position than the other parts of their organization would perhaps warrant them to take.

There is obviously a representative relationship between the crustaceous *Macrura*, *Anomura*, and *Brachyura* and the chitinous *Myriopoda*, *Insecta*, and *Arachnida*.

The earthworms and the leeches may help to fill up the gap between the *Chaetopod Annelida* and the *Myriopoda* (as, for example, between the genera *Geophilus* and *Nereis*), though it must be confessed that the existing links are inadequate, or they have never been sufficiently made out.

The first rudiments of a tracheal system are probably to be sought for in the *Terricolous Annelida*, though true articulated limbs and a dorsal heart seem to make their first appearance in the *Iulidæ*.

Should the simplest hydroid polyps have sprung from such Protozoa as *Diffugia*, *Arcella*, or *Astrorhiza*, with their pseudopodial tentacula encircling a fixed oral point, the existence of a living series from the lowest type of animals to that which is obviously on the confines of the *Vertebrata* would be clearly demonstrable*. Furthermore, as the interpolation of any other invertebrate types would disturb the harmony here, the inference is natural that they also might be distributed in a similar way into as many groups or series as their affinities or antipathies would suggest or necessitate.

Having studied this subject very carefully, it appears to me that the whole of the *Invertebrata* admit of distribution into four distinct series, corresponding with the number of sections of the Protozoa, from which all the other types may have taken their origin. Thus, on dividing the *Astomatous Protozoa* into compound types and their allied simple forms, we obtain the following highly suggestive arrangement, in which the groups represent each other so remarkably that they would seem to be quite natural. I have appended the *Stomatoda* and the twelve remaining sections of the *Invertebrata* in the order indicated by their affinities.

* The annexed Table exhibits the progressive modification of the alimentary system in ascending from the *Hydrozoa* to the *Tunicata* :—

Evolution of the Alimentary Canal in particular.

MOLLUSCOIDA (including Ctenophora).....	Intestine insulated from the somatic cavity ...	With primary hæmal and final neural flexure	} <i>Ascidiozoa</i> .
		With simple neural flexure	
	Intestine straight, and communicating with the somatic cavity		} <i>Brachiopoda</i> and <i>Polyzoa</i> .
	Intestine not yet developed; stomach communicating with the somatic cavity		
CŒLEENTERATA.....	True stomach not yet developed, its office being answered by the somatic cavity		} <i>Ctenophora</i> .
			} <i>Actinozoa</i> .
			} <i>Hydrozoa</i> .

Additional matter in the above connexion will be found in a paper by the author 'On the Morphological Relationships of the Molluscoïda and Cœlenterata,' published, in the *Transactions of the Royal Society of Edinburgh*, vol. xxiii. part 3, 1864.

*Scheme of Classification of Invertebrata.**Leading Types of Protozoa, aggregate or compound.*

1.	2.	3.	4.
Collophæra.	Porifera.	Polycystina.	Foraminifera.

Corresponding simple forms.

Thalassicolla.	Actinophrys.	Acanthometra.	Amœba.
Gregarina.	Gromia.	Podocyrthis.	Diffugia.

Derivative types.

Cestodea.	Infusoria.	Noctilucidæ.	Cœlenterata.
Trematoda.	Turbellaria.	Rotifera.	Molluscoïda.
Nematoidea.	Sipunculidæ.	Annelida.	Mollusca.
	Echinodermata.	Articulata.	

So as not to complicate the Table, I thought it better to supplement it with the definition of the four leading types of compound Protozoa.

1. In the Collophæra type, the sarcode-bodies lie at some distance apart and are always distinct.

2. In the Porifera type the sarcode-bodies are closely approximated or confluent.

3. In the Polycystina type the sarcode-bodies are concentric and connected by radiating stolons.

4. In the Foraminifera type the sarcode-bodies are connected by stolons in linear series or some order of juxtaposition.

If it is incumbent upon the developmental hypothesis to derive the Vertebrata from the preexisting Invertebrata, the only line through which it would be possible to trace their descent is that leading from the Protozoa to the Mollusca proper, or the fourth series of the Table. It would also appear that the Entozoa, Echinodermata, and Articulata appertain severally to separate series of their own; and whatever may happen by-and-by, it would be difficult to find, in the present fauna of the globe, a single form clearly deducible from any of them.

The habit of life of the Entozoon, the peculiarity of structure of the Echinoderm, and the very perfection of organization of the Articulata, as it were, preclude their evolution into any other existing type. To use a common phrase, they may be said to lead nowhere, though they may be easily and, I think, consistently traced back to their possible origin in the Protozoa.

It would be great presumption to say that even an approach to perfection had been attained in this attempted classification of a whole subkingdom of animals. Nevertheless in the preceding Table the relationships existing amongst the members of that subkingdom are presented to the eye at a single glance, and in a manner that would be quite unattainable by systems maintaining the original creation of every so-called species, and that in an order perhaps more easily described than understood.

- II. "On the Temperature at which *Bacteria*, *Vibriones*, and their Supposed Germs are killed when immersed in Fluids or exposed to Heat in a moist state." By H. CHARLTON BASTIAN, M.A., M.D., F.R.S., Professor of Pathological Anatomy in University College, London. Received March 4, 1873.

For more reasons than one we may, perhaps, now look back with advantage upon the friendly controversy carried on rather more than a century ago between the learned and generous Abbé Spallanzani and our no less distinguished countryman Turberville Needham. Writing concerning his own relation to Needham, the Abbé said*, "I wish to deserve his esteem whilst combating his opinion"; and, in accordance with this sentiment, we find him treating his adversary's views with great respect, and at the same time repudiating much of the empty and idle criticism in which so many of Needham's contemporaries indulged with regard to his work. This criticism, Spallanzani says†, "Without looking into details, contented itself by throwing doubt upon some of the facts, and by explaining after its own fashion others whose possibility it was willing to admit." He moreover warmly reprobated the ignorant and disrespectful statements made by an anonymous writer who had shown himself little worthy of being heard upon the subjects in dispute. Spallanzani on this occasion very wisely said ‡:—"When it is a question concerning observations and experiments, it is necessary to have repeated them with much circumspection before venturing to pronounce that they are doubtful or untrustworthy. He who will allow himself to speak of them with contempt, and who can only attempt to refute them with writings composed by the glimmer derived from a treacherous lamp, will not find himself in a condition to retain the esteem of learned men." The anonymous writer (in his '*Lettres à un Américain*'), to whom Spallanzani referred, had gone so far as to doubt the statements of Needham as to the constant appearance of organisms in infusions which had been previously boiled, and also intimated that even if they were to be found, it was only because they had been enabled to resist the destructive influence of the boiling fluid. This latter assertion was emphatically denied by Spallanzani, his denial being based upon a most extensive series of experiments with eggs in great variety and with seeds of all degrees of hardness; these were all found to be killed by a very short contact with boiling water. Spallanzani had thoroughly satisfied himself that even very thick-coated seeds could not resist this destructive agent; whilst he thought that the

* *Nouvelles Recherches sur les Découvertes Microscopiques et la Génération des Corps Organisés*, &c. London and Paris, 1769, vol. i. p. 69.

† *Loc. cit.* p. 9.

‡ *Loc. cit.* p. 114.

idea, entertained by some, of the eggs of the lowest infusoria being protected from the injurious influence of the boiling water by reason of their extreme minuteness, was a supposition so improbable as scarcely to deserve serious consideration. Such a notion was, he thought, wholly opposed to what was known concerning the transmission of heat. Whilst, therefore, the opinion of those who believe that eggs have the power of resisting the destructive influence of boiling water could be fully refuted, Spallanzani thought it by no means followed that the infusoria which always, after a very short time, appeared in boiling infusions had arisen independently of the existence of eggs. The infusions being freely exposed to the air, it was very possible that this air had introduced eggs into the fluids, which by their development had given birth to the infusoria*.

After the lapse of a century it has at last been clearly shown that this supposition of aërial contamination advanced by Spallanzani (warrantable and natural as it was at the time) is one which, in the great majority of cases, is devoid of all foundation in fact, so far as concerns the organisms essentially associated with processes of putrefaction, viz. *Bacteria* and *Vibriones*. The means of proving this statement, based upon independent observations made by Professor Burdon Sanderson and myself, were recently submitted to the consideration of the Royal Society†. Before the reading of this communication I was under the impression that almost every one of those who had taken part in the controversies which had been carried on both here and abroad concerning the Origin of Life were prepared to admit, as Spallanzani had done, that the eggs or germs of such organisms as appear in infusions were unable to survive when the infusions containing them were raised to the temperature at which water boils. This impression was produced in part by the explicit statements on this subject that had been made by very many biologists, and also in part by a comparatively recent and authoritative confirmation which this view as to the destructive effects of boiling infusions upon *Bacteria* had received. Little more than two years ago Professor Huxley, as President of the British Association for the Advancement of Science, recorded experiments in his Inaugural Address which were obviously based upon this belief as a starting-point; and subsequently, in one of the Sectional Meetings, after referring to some of my experiments, and to the fact that all unmistakably vital movements ceased after *Bacteria* had been boiled, Professor Huxley added‡:—"I cannot be certain about other persons, but I am of opinion that

* A few pages further on this view is thus shortly expressed:—"Il est évident que toutes les tentatives faites avec le feu, peuvent bien servir à prouver que les animaux microscopiques ne naissent point des œufs que l'on supposait exister dans les infusions avant qu'on leur fit sentir le feu; mais cela n'empêche pas qu'ils n'aient pu être formés de ceux qui auront été portés dans les vases après l'ébullition."

† See Proceedings of Royal Society, No. 141, 1873, p. 129.

‡ See Report in Quart. Journ. of Microscop. Science, Oct. 1870.

observers who have supposed they have found *Bacteria* surviving after boiling have made the mistake which I should have done at one time, and, in fact, have confused the Brownian movements with *true living* movements." Some eminent biologists do not now (in reference to the experiments cited in my last communication) suggest that the organisms found in the infusions were dead and had been there before the fluids were boiled: they express doubts concerning that which seems formerly to have been regarded as established, and now wish for evidence to show that the germs of *Bacteria* and *Vibriones* are killed in a boiling infusion of hay or turnip, as they have been proved to be in "Pasteur's Solution" and in solutions containing ammoniac tartrate and sodic phosphate.

With the view of removing this last source of doubt more effectually, and also of refuting the unwarrantable* conclusion of M. Pasteur, to the effect that the germs of *Bacteria* and *Vibriones* are not killed in neutral or slightly alkaline fluids at a temperature of 212° F., I almost immediately after the reading of my last communication commenced a fresh series of experiments.

Nearly two years ago, in my 'Modes of Origin of Lowest Organisms,' I brought forward evidence to show that *Bacteria*, *Vibriones*, and their supposed germs are killed at a temperature of 140° F. (60° C.) in neutral or very faintly acid solutions containing ammoniac tartrate and sodic phosphate, and also evidence tending to show that these living units were killed in neutral infusions of hay and in acid infusions of turnip at the same temperature.

The crucial evidence adduced concerning the degree of heat destructive to *Bacteria*, *Vibriones*, and their germs, in the saline solution, was of this nature. The solution had been shown to be incapable of engendering *Bacteria* and *Vibriones* (under all ordinary conditions) after it had been boiled, although it still continued capable of supporting the life and encouraging the rapid multiplication of any of these organisms which were purposely added to it. Some of this boiled solution, therefore, was introduced into flasks previously washed with boiling water; and when the fluids had sufficiently cooled, that of each flask was inoculated with living *Bacteria* and *Vibriones*—in the proportion of one drop of a fluid quite turbid with these organisms to one fluid ounce of the clear saline solution†. These mixtures containing an abundance of living organisms were then heated to various temperatures, ranging from 122° F. (50° C.) to 167° F. (75° C.); and it was invariably found that those which had been

* Reasons for this opinion have been fully set forth in 'The Beginnings of Life,' vol. i. pp. 374 *et seq.*; or the discriminating reader may at once find my justification for this expression by reading pp. 58–66 of M. Pasteur's memoir in 'Ann. de Chim. et de Physique,' 1862.

† Fuller details concerning these experiments may be found in the little work already mentioned at pp. 51–56, and also in 'The Beginnings of Life,' vol. i. pp. 325–332.

heated to 122° or 131° F. became quite turbid in about two days, whilst those which had been raised to 140° F. or upwards as invariably remained clear and unaltered. The turbidity in the first series having been ascertained to be due to the enormous multiplication of *Bacteria* and *Vibriones*, and it being a well-established fact that such organisms when undoubtedly living always rapidly multiply in these fluids, the conclusion seemed almost inevitable that the organisms and their germs must have been killed in the flasks which were briefly subjected to the temperature of 140° F. How else are we to account for the fact that these fluids remained quite unaltered although living organisms were added to them in the same proportion as they had been to those less-heated fluids which had so rapidly become turbid? Even if there does remain the mere possibility that the organisms and their supposed germs had not actually been killed, they were certainly so far damaged as to be unable to manifest any vital characteristics. The heat had, at all events, deprived them of their powers of growth and multiplication; and these gone, so little of what we are accustomed to call "life" could remain, that practically they might well be considered dead. And, as I shall subsequently show, the production of this potential death by the temperature of 140° F. enables us to draw just the same conclusions from other experiments, as if such a temperature had produced a demonstrably actual death*. Seeing also that these saline solutions were inoculated with a fluid in which *Bacteria* and *Vibriones* were multiplying rapidly, we had a right to infer that they were multiplying in their accustomed manner, "as much by the known method of fission, as by any unknown and assumed method of reproduction." So that, as I at the time said†, "These experiments seem to show, therefore, that even if *Bacteria* do multiply by means of invisible gemmules, as well as by the known process of fission, such invisible particles possess no higher power of resisting the destructive influence of heat than the parent *Bacteria* themselves possess."

This is, in fact, by far the most satisfactory kind of evidence that can be produced concerning the powers of resisting heat enjoyed by *Bacteria* and *Vibriones*, because it also fully meets the hypothesis as to their possible multiplication by invisible gemmules possessed of a greater power of resisting heat, and because no mere inspection by the microscope of dead *Bacteria* can entitle us positively to affirm that they are dead, even though all characteristically vital or "true living" movements may be absent.

Facts of a very similar nature were mentioned in the same work strongly tending to show that *Bacteria* and *Vibriones* are also killed at the same temperature in other fluids, such as infusions of hay or turnip. These facts were referred to in the following statement‡:—"Thus, if on

* See p. 232.

† *Modes of Origin of Lowest Organisms*, 1871, p. 60.

‡ *Loc. cit.* p. 60.

the same slip, though under different covering-glasses, specimens of a hay-infusion turbid with *Bacteria* are mounted, (a) without being heated, (b) after the fluid has been raised to 122° F. for ten minutes, and (c) after the fluid has been heated to 140° F. for ten minutes, it will be found that in the course of a few days the *Bacteria* under a and b have notably increased in quantity, whilst those under c do not become more numerous, however long the slide is kept. Facts of the same kind are observable if a turnip-infusion containing living *Bacteria* is experimented with; and the phenomena are in no way different if a solution of ammoniac tartrate and sodic phosphate (containing *Bacteria*) be employed instead of one of these vegetable infusions. The multiplication of the *Bacteria* beneath the covering-glass, when it occurs, is soon rendered obvious, even to the naked eye, by the increasing cloudiness of the film."

The facts just cited concerning the behaviour of thin films of turbid infusions which had been heated to different temperatures gave me the clue as to the proper direction of future work. It would seem that, when mounted in the manner described, such thin films of infusion continue capable of supporting and favouring the multiplication of any already existing *Bacteria* and *Vibriones*, although under such conditions no new birth of living particles appears to take place even in these fluids. The question then arose as to whether, by subjecting larger quantities of the same infusions to any particular sets of conditions, we could ensure that they also should continue to manifest the same properties; because if so, it would be almost as easy to determine the death-point of *Bacteria* and *Vibriones* when exposed to heat in these infusions, as it had been to determine it for the saline solutions already mentioned.

It was pointed out by Gruithuisen early in the present century, that many infusions, otherwise very productive, ceased to be so when they were poured into a glass vessel whilst boiling, and when this was filled so that the tightly fitting stopper touched the fluid. Having myself proved the truth of this assertion for hay-infusion, it seemed likely that, by having recourse to a method of this kind, I should be able to lower the virtues of boiled hay- and turnip-infusions to the level of those possessed by the boiled saline solution with which I had previously experimented—that is, to reduce them to a state in which, whilst they appear (under these conditions) quite unable of themselves to engender *Bacteria* or *Vibriones*, they continue well capable of favouring the rapid multiplication of such organisms.

This was found to be the case; and I have accordingly performed upwards of one hundred experiments with inoculated portions of these two infusions raised to different temperatures. The mode in which the experiments were conducted was as follows:—

Infusions of hay and turnip of slightly different strengths were employed. These infusions, having been first loosely strained through

muslin, were boiled for about ten or fifteen minutes, and then whilst boiling strained through ordinary Swedish filtering-paper into a glass beaker which had previously been well rinsed with boiling water. A number of glass bottles or tubes were also prepared, which, together with their stoppers or corks, had been boiled in ordinary tap water for a few minutes*. They were taken out full of the boiling fluid; and the stoppers or corks being at once inserted, the vessels and their contents were set aside to cool. When the filtered infusion of hay or turnip had been rapidly cooled down to about 110° F. (by letting the beaker containing it stand in a large basin of cold water), it was inoculated with some of a turbid infusion of hay swarming with active *Bacteria* and *Vibriones*—in the proportion of one drop of the turbid fluid to each fluid ounce of the now clear filtered infusion†. The beaker was then placed upon a sand-bath, and its contained fluid (in which a thermometer was immersed) gradually raised to the required temperature. The fluid was maintained at the same temperature for five minutes by alternately raising the beaker from and replacing it upon the sand-bath. The bottles to be used were then one by one uncorked, emptied, and refilled to the brim with the heated inoculated fluid‡. The corks or stoppers were at once very tightly pressed down, so as to leave no air between them and the surface of the fluids. The beaker was then replaced upon the sand-bath and the gas turned on more fully, in order that the experimental fluid might be rapidly raised to a temperature 9° F. (5° C.) higher than it had been before. After five minutes' exposure to this temperature other bottles were filled in the same manner, and so on for the various temperatures the influence of which it was desired to test.

Thus prepared, the bottles and tubes have been exposed during the day to a temperature ranging from 65° to 75° F. And generally one had not to wait long in order to ascertain what the results were to be. In some cases, if the contents of the vessels were to become turbid, this was more or less manifest after an interval of forty-eight hours; in other cases, however, the turbidity manifested itself three or more days later: the reason of this difference will be fully discussed in a subsequent communication.

For the sake of simplicity and brevity, the necessary particulars concerning the 102 experiments have been embodied in the following Table:—

* The vessels employed have varied in capacity from two drachms to four ounces; some have been provided with glass stoppers and others with very tightly fitting corks; and the latter I find have answered quite as well as the former. On the whole I have found tightly corked one-ounce phials to be about the most convenient vessels to employ in these inoculation experiments.

† It was found desirable to filter the infusions after they had been boiled, because the boiling generally somewhat impaired their clearness.

‡ At this stage, of course, *very great care* is needed in order to avoid all chance of accidental contamination either with living organisms or with unheated fragments or particles of organic matter.

Inoculation Experiments made with the view of ascertaining the Temperatures at which *Bacteria*, *Vibriones*, and their supposed Germs are killed in Organic Infusions.

NEUTRAL HAY-INFUSION.			
Temp. to which exposed.	Number of experiments made.	Date of Turbidity, if any.	Results at Expiration of the 8th day.
122° F. } (50° C.) } 131° F.	1 7	24 hours. 48 hours.	Turbid. All turbid.
140° F.	9	{ 1 in 48 hours. 6 in 60 hours. 1 in 3 days. 1 in 8 days.	All turbid.
149° F.	4	{ 2 in 5 days. 1 in 8 days.	{ Three turbid. One clear.
158° F.	15	All clear.
167° F.	4	All clear.
176° F. } (80° C.) }	12	All clear.

ACID TURNIP-INFUSION.			
Temp. to which exposed.	Number of experiments made.	Date of Turbidity, if any.	Results at Expiration of the 8th day.
122° F. 131° F. 7 { 5 in 24 hours. 2 in 48 hours. } All turbid.
140° F.	12	{ 6 in 40 hours. 4 in 3 days. 2 in 4 days.	All turbid.
149° F.	10	{ 1 in 3 days. 3 in 5 days. 1 in 7 days. 2 in 8 days. }	{ Seven turbid. Three clear.
158° F.	17	All clear.
167° F.	4	All clear.
176° F.

The experimental results above tabulated seem naturally divisible into three groups. Thus, when heated only to 131° F., all the infusions became turbid within two days, just as the inoculated saline solutions had done*. Heated to 158° F. all the inoculated organic infusions remained clear, as had been the case with the saline solutions in my previous experiments when heated to 140° F. There remains, therefore, an intermediate heat zone (ranging from a little below 140° to a little below 158° F.) after an exposure to which the inoculated organic infusions are apt to become more slowly turbid, although inoculated saline solutions raised to the same temperatures invariably remain unaltered. The full explanation of these apparent anomalies I propose to make the subject of a future communication to the Royal Society; meanwhile we may quite safely conclude that *Bacteria*, *Vibriones*, and their supposed germs are either actually killed or else completely deprived of their powers of multiplication after a brief exposure to the temperature of 158° F. (70° C.).

This evidence now in our possession as to the limits of "vital resistance" to heat displayed by *Bacteria*, *Vibriones*, and their supposed germs in neutral saline solutions, and in neutral or acid organic infusions, is most pertinent and valuable when considered in relation to that supplied by other sets of experiments bearing upon the all-important problem of the Origin of Life. These latter experiments alone may possibly leave doubt in many minds; but the more thoroughly they are considered in relation to the evidence brought forward in this communication, the more fully, I venture to think, will every lingering doubt as to the proper conclusion to be arrived at be dispelled.

Thus we now know that boiled turnip- or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days; but, placed under slightly different conditions such as were employed in the inoculation experiments above quoted, although infusions of the same nature do not undergo "spontaneous" putrefactive changes, yet when living *Bacteria* and *Vibriones* are added, and not subsequently heated, putrefaction invariably takes place and the fluids thus situated rapidly become turbid. There is therefore nothing in the conditions themselves tending to hinder the process of putrefaction, so long as living units are there to initiate it. Our experiments now show that as long as the added *Bacteria*, *Vibriones*, and their supposed germs are subjected to a heat not exceeding 131° F. (55° C.), putrefaction invariably occurs within two days; whilst, on the contrary, whenever they are subjected to a temperature of 158° F. (70° C.) putrefaction does not occur. To what can this difference be due, except to the fact that the previously living organisms, which, when living, always excite putrefaction, have been

* In the experiments already referred to.

killed by the temperature of 158° F.? It would be of no avail to suppose that the absence of putrefaction in these latter cases is due to the fact that a heat of 158° F., instead of killing the organisms and their germs, merely annuls their powers of reproduction, because in the other series of experiments (with which these have to be compared), where similar fluids are exposed to ordinary or purified air, or are shut off from the influence of air altogether, the most active putrefaction and multiplication of organisms takes place in two, three, or four days, in spite of the much more potent heat of 212° F. to which any preexisting germs or organisms must have been subjected. The supposition, therefore, that the *Bacteria*, *Vibriones*, and their germs were not killed in our inoculation experiments at the temperature of 158° F., but were merely deprived of their powers of reproduction, would be no gain to those who desire to stave off the admission that *Bacteria* and *Vibriones* can be proved to arise *de novo* in certain cases. Let us assume this (which is indisputably proved by these inoculation experiments), viz. that an exposure to a temperature of 158° F. (70° C.) for five minutes deprives *Bacteria*, *Vibriones*, and their germs of their usual powers of growth and reproduction—that is, that it reduces them to a state of potential, if not necessarily to one of actual death. What end would be served by such a reservation? The impending conclusion could not be staved off by means of it. The explanation of what occurs in the other set of experiments, where the much more potent heat of 212° F. is employed, still would not be possible without having recourse to the supposition of a *de novo* origination of living units, so long as those which may have pre-existed in the flasks could be proved to have been reduced to such a state of potential death. It would be preposterous, and contrary to the whole order of Nature, to assume that the vastly increased destructive influence of a heat of 212° F. had restored vital properties which a lesser amount (158° F.) of the same influence had completely annulled.

The evidence supplied by these different series of experiments, in whichever way it is regarded, as it seems to me, absolutely compels the logical reasoner to conclude that the swarms of living organisms which so often make their appearance in boiled infusions treated in one or other of the various modes already proved to be either destructive or exclusive of preexisting living things are the products of a new brood of “living” particles, which, in the absence of any coexisting living organisms, must have taken origin in the fluid itself. For this mode of origin of living units, so long spoken of and repudiated as “spontaneous generation,” I have proposed the new term Archebiosis.

III. "Some new Theorems on the Motion of a Body about a Fixed Point." By EDWARD JOHN ROUTH, M.A., F.R.S. Received December 21, 1872.

Poinsot constructs the motion of a rigid body about a fixed point under the action of no forces by means of an ellipsoid which has its centre at the fixed point and rolls on a fixed plane. In this manner the relations of the invariable line, and the instantaneous axis to each other and to the other parts of the body, may be found by solid geometry. It is evident that in many cases these relations are merely translations into the language of solid geometry of certain properties of the spherical ellipse. In trying to make use of the spherical ellipse in a general manner, I have been led to some theorems which I think are interesting, and which seem to be new. In what follows I shall restrict myself almost entirely to these results.

1. Let a body be turning about a fixed point O under the action of no forces, and let the angular velocities about the principal axes at O be $\omega_1, \omega_2, \omega_3$. Also let A, B, C be the principal moments at the fixed point; then we know that if we put

$$\left. \begin{aligned} \omega_1^2 + \omega_2^2 + \omega_3^2 &= \omega^2, \\ A\omega_1^2 + B\omega_2^2 + C\omega_3^2 &= T, \\ A^2\omega_1^2 + B^2\omega_2^2 + C^2\omega_3^2 &= G^2, \end{aligned} \right\}$$

T and G are constant throughout the motion, and ω is the resultant angular velocity.

2. There are three straight lines whose motions we may consider:—

(1) the Instantaneous axis, whose direction cosines are $\frac{\omega_1}{\omega}, \frac{\omega_2}{\omega}, \frac{\omega_3}{\omega}$; (2)

the Invariable axis, whose direction cosines are $\frac{A\omega_1}{G}, \frac{B\omega_2}{G}, \frac{C\omega_3}{G}$; (3) the

Eccentric axis, whose direction cosines are $\frac{\sqrt{A}\omega_1}{\sqrt{T}}, \frac{\sqrt{B}\omega_2}{\sqrt{T}}, \frac{\sqrt{C}\omega_3}{\sqrt{T}}$. This

last axis is the eccentric line of the instantaneous axis with regard to the momental ellipsoid at the fixed point, or, which is the same thing, the eccentric line of the invariable axis with regard to the ellipsoid of gyration. Let a sphere of radius unity be described whose centre is O, fixed in the body and therefore moving with it. Let these three axes cut the sphere respectively in the points I, G, H. Let the principal axes at O cut the sphere in A, B, C. It is our object to discuss the motions of I, G, H relatively to A, B, C and in space.

3. The equations to the cones described in the body by OI, OG, OH respectively are

$$\left. \begin{aligned} A(AT - G^2)x^2 + B(BT - G^2)y^2 + C(CT - G^2)z^2 &= 0, \\ \frac{AT - G^2}{A}x^2 + \frac{BT - G^2}{B}y^2 + \frac{CT - G^2}{C}z^2 &= 0, \\ (AT - G^2)x^2 + (BT - G^2)y^2 + (CT - G^2)z^2 &= 0. \end{aligned} \right\}$$

It is clear that the three points I, G, H describe spherical ellipses on the sphere of radius unity. We know that $\frac{G^2}{T}$ lies between the greatest, A, and the least, C, of the principal moments of inertia; let us suppose that it is also greater than the mean principal moment B. In this case the concavities of the three ellipses will be towards the point A, i. e. the point where the axis of greatest moment cuts the sphere. This point is therefore to be regarded as the common centre of the three ellipses. If $\frac{G^2}{T} < B$, the common centre would be the intersection of the least axis of moment with the sphere.

In the figure the eye is supposed situated on the axis of greatest moment, viewing the sphere from a considerable distance. All great circles on the sphere are represented by straight lines.

The semiaxes A K, A L of the eccentric ellipse are evidently given by $\cotan \alpha = \sqrt{\frac{G^2 - BT}{AT - G^2}}$ and $\cotan \beta = \sqrt{\frac{G^2 - CT}{AT - G^2}}$. If (a', b') , (a, b) be the semiaxes of the instantaneous ellipse and invariable ellipse respectively, we have

$$\frac{\tan a}{B} = \frac{\tan a'}{A} = \frac{\tan \alpha}{\sqrt{AB}},$$

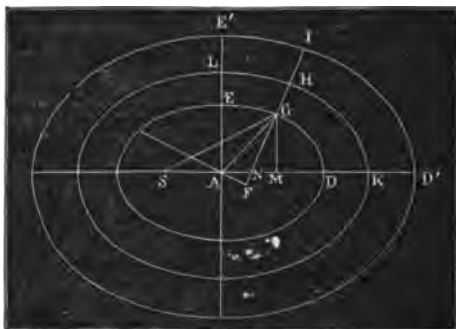
$$\frac{\tan b}{C} = \frac{\tan b'}{A} = \frac{\tan \beta}{\sqrt{AC}}.$$

Thus it appears that the *ratios of the tangents of the semiaxes are independent of the initial conditions.*

If we put $1 - e^2 = \frac{\sin^2 b}{\sin^2 a}$, we may define e to be the eccentricity of the invariable ellipse. If ϵ be the eccentricity of the eccentric ellipse, we easily find

$$e^2 = \frac{A}{B} \frac{B-C}{A-C}, \quad \epsilon^2 = \frac{B-C}{A-C};$$

so that *both these eccentricities are independent of the initial conditions.*



4. Since the body is turning about O I as instantaneous axis, it is evident that the motion of G in the body is perpendicular to the great circle I G. Hence I G produced is a normal to the invariable ellipse.

5. If ω be the angular velocity of the body, the angular velocity of O G is $\omega \sin \angle G I$. But the resolved angular velocity of the body about O G is constant, and $= \frac{T}{G}$ by a theorem due to Lagrange. Hence the velocity of the invariable line along its ellipse is $\frac{T}{G} \tan \angle G I$. Also the resultant angular velocity of the body is $\frac{T}{G \cos \angle G I}$.

6. Let the great circle G I cut the axis major in N, so that G N is a normal to the invariable ellipse. Referring the lines to the principal axes at the fixed point O as coordinate axes, the direction cosines of O G, O N are easily seen to be proportional to

$$\begin{array}{ccc} A\omega_1, & B\omega_2, & C\omega_3, \\ (A-C)\omega_1, & (B-C)\omega_2, & 0. \end{array}$$

Hence

$$\cos \angle G N = \frac{A(A-C)\omega_1^2 + B(B-C)\omega_2^2}{G \cdot \sqrt{(A-C)^2\omega_1^2 + (B-C)^2\omega_2^2}}.$$

Substituting for ω_1, ω_2 their known values in terms of ω ,

$$\tan \angle G N = \frac{C\sqrt{G^2\omega^2 - T^2}}{G^2 - CT}.$$

But $\omega \cos \angle G I = \frac{T}{G}$; $\therefore \tan \angle G I = \frac{\sqrt{G^2\omega^2 - T^2}}{T}.$

It follows at once that the ratio $\frac{\tan \angle G I}{\tan \angle G N}$ is constant throughout the motion and $= \frac{G^2 - CT}{CT}.$

It also follows that the extremity of the invariable line moves along its ellipse with a velocity which bears a constant ratio to the tangent of the length of the normal to the ellipse intercepted between the curve and either axis.

7. Let us find the motion of the invariable axis referred to one focus, S, of the ellipse described by it.

The velocity of G resolved perpendicular to the focal radius vector S G is, by what precedes,

$$= \frac{G^2 - CT}{CG} \tan \angle G N \cdot \cos \angle S G N.$$

It may be shown that in any spherical ellipse the projection of the normal on either focal radius is constant. If this constant be called l , we have $\tan l = \frac{\tan^2 b}{\tan a}.$

If, therefore, NL be a perpendicular arc drawn from N on SG, we have

$$\tan GL = \tan GN \cos SGN.$$

Hence the resolved velocity of G

$$\begin{aligned} &= \frac{G^2 - CT}{CG} \tan l \\ &= \frac{1}{G} \sqrt{\frac{(G^2 - BT)(AT - G^2)}{AB}}. \end{aligned}$$

Hence throughout the motion the extremity G of the invariable axis moves along its ellipse in such a manner that its velocity resolved perpendicular to the focal radius vector SG is constant.

8. Let us next find the velocity of G resolved along the focal radius vector SG. Let $SG = \rho$, $GSA = \theta$; then the polar equation to a spherical ellipse may be proved to be

$$\frac{\sin a \cos a (1 - e^2)}{\tan \rho} = \cos^2 b - e \cos \theta.$$

Differentiating this, we get

$$\frac{(1 - e^2) \sin a \cos a}{\sin^2 \rho} \frac{d\rho}{dt} = -e \sin \theta \frac{d\theta}{dt}.$$

But the value of $\sin \rho \frac{d\theta}{dt}$, being the velocity of G perpendicular to the radius vector SG, has just been found. Also if GM be drawn as an ordinate perpendicular to the major axis, we have $\sin \rho \sin \theta = \sin GM$. Hence, substituting from art. 3, we get

$$\frac{d\rho}{dt} = -\frac{G}{C} \sqrt{\frac{(A - C)(B - C)}{AB}} \sin GM.$$

Hence the velocity of G resolved along either focal radius vector bears to the sine of the ordinate of G a ratio which is constant throughout the motion. This constant, when divided by the resultant angular momentum, is independent of the initial conditions.

9. We may also refer the motion of G to the centre of its ellipse.

The velocity of G along its ellipse being $\frac{G^2 - CT}{CG} \tan GN$, the resolved velocity perpendicular to AG is

$$= \frac{G^2 - CT}{CG} \tan GN \cdot \cos AGN.$$

But if AF be an arc drawn from A perpendicular to GN, we have, by Napier's rules,

$$\cos AGN = \tan GF \cdot \cot AG;$$

and in any spherical ellipse we have

$$\tan GN \cdot \tan GF = \tan^2 b.$$

Eliminating $\cos AGN$ and $\tan GF$, we have

$$\text{resolved velocity of } G = \frac{AT - G^2}{AG} \cot AG.$$

Hence the velocity of G resolved perpendicular to its central radius vector varies as the cotangent of that radius vector.

If GM , GM' be perpendicular arcs drawn from G on the axes, we may show that the velocity of G resolved along its central radius vector is $= -G \frac{B-C}{BC} \cdot \frac{\sin GM \cdot \sin GM'}{\sin AG}$. This result may be deduced from one of Euler's equations.

10. The relations of the motions of the points G , H , I to one another may easily be deduced from the following simple formulæ.

If we eliminate ω_1 , ω_2 , ω_3 from the expressions for the direction cosines of OG , OH , OI , and ω by Lagrange's theorem, $\omega \cos GI = \frac{T}{G}$, we find

$$\frac{\cos AI}{G^2 \cos GI} = \frac{\cos AG}{AT} = \frac{\cos AH}{G \sqrt{AT}}.$$

If P be any point on the sphere, we have, by Napier's rules,

$$\frac{\cos BP}{\cos CP} = \frac{\cos BAP}{\cos CAP} = \cot PAB.$$

From this we easily deduce

$$\frac{\tan \theta'}{B} = \frac{\tan \theta}{C} = \frac{\tan \theta''}{\sqrt{BC}},$$

where θ' , θ , θ'' are the angles the central radii vectores AI , AG , AH make with the major axis.

11. Let us apply these to find the motion of the extremity I of the instantaneous axis along its ellipse.

Differentiating the expression $\tan \theta' = \frac{B}{C} \tan \theta$, we get

$$\frac{d\theta'}{dt} = \frac{B \cos^2 \theta'}{C \cos^2 \theta} \frac{d\theta}{dt}.$$

If (α, β, γ) , $(\alpha', \beta', \gamma')$ be the direction angles of OG and OI referred to the principal axes at O , we have, by Napier's rules, $\cos \theta = \frac{\cos \beta}{\sin \alpha}$ and

$\cos \theta' = \frac{\cos \beta'}{\sin \alpha'}$. Also $\frac{d\theta}{dt}$ has been already found. Hence

$$\frac{d\theta'}{dt} = \frac{\cos^2 \beta'}{\sin^2 \alpha'} \cdot \frac{\sin^2 \alpha}{\cos^2 \beta} \cdot \frac{AT - G^2}{AG} \frac{1}{\tan \alpha \cdot \sin \alpha} \frac{B}{C};$$

$$\therefore \sin \alpha' \frac{d\theta'}{dt} = \frac{G}{T} \frac{AT - G^2}{BC} \cdot \cot \alpha' \cdot \cos GI.$$

This gives the velocity perpendicular to AI . The velocity along its

ellipse may be found by dividing this by $\cos \angle IN'$, where IN' is a normal to the instantaneous axis cutting the major axis in N' . By a property of the spherical ellipse proved above, we have $\cos \angle IN' = \frac{\tan^2 b'}{\tan \angle AI \cdot \tan n''}$.

Hence, substituting,

$$\left. \begin{array}{l} \text{velocity of I} \\ \text{along its ellipse} \end{array} \right\} = \frac{G}{T} \frac{G^2 - CT}{AB} \tan n' \cdot \cos \angle GI,$$

where n' is the length of the normal to the instantaneous ellipse intercepted between the curve and the major axis.

12. If we compare this formula with the corresponding formula for the velocity of G , viz.

$$\left. \begin{array}{l} \text{velocity of G} \\ \text{along its ellipse} \end{array} \right\} = \frac{G^2 - CT}{CG} \tan n,$$

we see that for every theorem relating to the motion of G in its ellipse there is a corresponding theorem for the motion of I . For example, if S' be a focus of the instantaneous ellipse, we see that the velocity of I resolved perpendicular to the focal radius vector $S'I$ bears a constant ratio to $\cos \angle GI$.

This constant ratio is $= \frac{G}{CT} \sqrt{\frac{(AT - G^2)(G^2 - BT)}{AB}}$.

13. From the two formulæ for the velocities of G and I we may easily deduce

$$\frac{\text{angular velocity of I about A}}{\text{angular velocity of G about A}} = \frac{A^2 (\cot \angle AI)^2}{BC (\cot \angle AG)^2}.$$

14. We may apply the same kind of reasoning to find the motion of H along the eccentric ellipse. Beginning with $\tan \theta'' = \sqrt{\frac{B}{C}} \tan \theta$, we find

$$\text{velocity of H} = \frac{G^2 - CT}{\sqrt{ABCT}} \cdot \tan n'',$$

where n'' is the length of the normal to the eccentric ellipse intercepted between the curve and the major axis.

Hence the velocity of the eccentric line along its ellipse varies as the tangent of the normal to its path.

15. It may be proved that in any spherical ellipse

$$\tan^2 n = \frac{\tan^2 b}{\cos^2 b \sin^2 a} \cdot (\cos^2 r - \cos^2 a \cos^2 b),$$

where r is the central radius vector, and n the length of the normal. By using this formula we easily find

$$(\text{velocity of H})^2 - (\text{velocity of G})^2 = \text{constant}.$$

This constant is $= \frac{(G^2 - AT)(G^2 - BT)(G^2 - CT)}{ABCG^2T}$.

16. The invariable axis OG being fixed in space, it will be useful to find the motion of I relatively to G.

Let ρ be the radius of curvature of the spherical ellipse described by G, and let $GI = z$. Then clearly

$$\left. \begin{array}{l} \text{velocity of I resolved} \\ \text{perpendicularly to GI} \end{array} \right\} = \left(\begin{array}{l} \text{velocity} \\ \text{of G} \end{array} \right) \cdot \frac{\sin(\rho + z)}{\sin \rho}$$

$$= \frac{T}{G} \tan z \cdot (\cos z + \cot \rho \sin z).$$

But it may be proved that in any spherical ellipse

$$\tan \rho = \frac{\tan^2 n}{\tan^2 l};$$

where n is the length of the normal, and $2l$ the length of the latus rectum. Substituting for $\tan \rho$, and remembering that the ratio

$$\frac{\tan z}{\tan n} = \frac{G^2 - CT}{CT},$$

we get

$$\left. \begin{array}{l} \text{angular velocity} \\ \text{of I about G} \end{array} \right\} = \frac{T}{G} + \frac{T}{G} \left(\frac{G^2 - CT}{CT} \right)^3 \left(\frac{\tan^2 b}{\tan a} \right)^2 \cot^2 z,$$

where a and b are the semiaxes of the ellipse described by G. If we substitute for $\tan a$, $\tan b$ their values, we get

$$\left. \begin{array}{l} \text{angular velocity of} \\ \text{I in space about G} \end{array} \right\} = \frac{T}{G} + \frac{(AT - G^2)(BT - G^2)(CT - G^2)}{ABCGT^3} \cot^2 z,$$

which agrees with Poinso's results.

17. We may put this result into another form. Substituting in art. 15 for the velocity of G, viz. $\frac{T}{G} \tan z$, we get an expression for the velocity of H in terms of z . Comparing it with the above result, we get

$$\left. \begin{array}{l} \text{angular velocity of} \\ \text{I in space about G} \end{array} \right\} = \frac{G}{T} \cot^2 z \cdot (\text{velocity of H})^2.$$

18. The angles described in space round G by the arcs GA, GS may be represented geometrically thus:—

Let ψ be the angle described by GA; then, as proved in books on Rigid Dynamics,

$$\frac{d\psi}{dt} = \frac{T}{G} + \frac{AT - G^2}{AG} \cot^2 AG.$$

Let θ be the angle GAD, then it has been proved above (art. 9) that

$$\frac{d\theta}{dt} = \frac{AT - G^2}{AG} \cot^2 AG \cdot \sec AG;$$

$$\therefore \frac{d\psi}{dt} = \frac{T}{G} + \cos AG \cdot \frac{d\theta}{dt}.$$

Hence, integrating, we get

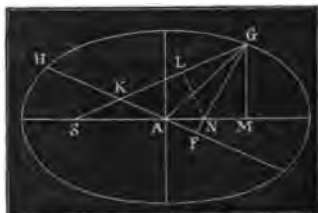
$$\psi = \frac{T}{G} t + \overset{\wedge}{GAD} - \left(\frac{\text{vectorial area}}{GAD} \right).$$

By subtracting the angle SGA, we get, if ψ' be the angle described by GS in space,

$$\psi' = \frac{T}{G} t + \overset{\wedge}{GSA} - \left(\frac{\text{vectorial area}}{GSD} \right).$$

APPENDIX.

The following properties of the spherical ellipse will be useful in connexion with what precedes. I have not been able to find them anywhere.



1. Equation to the ellipse,

$$\left. \begin{aligned} \tan y &= \frac{\tan b}{\tan a} \tan y', \\ \cos a &= \cos y' \cdot \cos x. \end{aligned} \right\}$$

Here y' is the eccentric ordinate and $\frac{\tan y}{\tan y'}$ is constant.

2. The projection of the normal GN on the focal radius vector SG, i. e. GL is constant and equal to semilatus rectum.

If l = semilatus rectum, $\tan l = \frac{\tan^2 b}{\tan a}$; also $\frac{\tan NL}{\sin GM} = \text{constant}$.

3. If HAF cut GN at right angles, $\tan GN \cdot \tan GF = \tan^2 b$.

4. The length GK cut off the focal radius vector by the conjugate diameter is constant and equal to a .

This follows from (2) and (3).

5. If $e^2 = 1 - \frac{\sin^2 b}{\sin^2 a}$, then GM being an ordinate perpendicular to AN,

$$\tan AN = e^2 \tan AM.$$

6. Also S being a focus,

$$\tan (SG - a) = e \tan AM.$$

7. Polar equation to the ellipse,

$$\frac{\tan l}{\tan SG} = 1 - \frac{e}{\cos^2 b} \cos \overset{\wedge}{GSA}.$$

8. If ρ be the radius of curvature,

$$\tan \rho = \frac{\tan^3 n}{\tan^3 l}.$$

9. Regarding AG, AH as conjugate diameters,

$$\begin{aligned}\sin^2 AH + \sin^2 AG &= \sin^2 a + \sin^2 b, \\ \sin AH \cdot \sin GF &= \sin a \cdot \sin b.\end{aligned}$$

10. If p be the perpendicular from A on the tangent at G,

$$\frac{\tan^2 a \tan^2 b}{\tan^2 p} = \tan^2 a + \tan^2 b - \tan^2 AG.$$

$$11. \tan^2 GN - \tan^2 l = \frac{e^2}{\cos^4 b} \sin^2 GM.$$

$$12. \left. \begin{aligned}\sin^2 a - \sin^2 AG \\ = \sin^2 HA - \sin^2 b\end{aligned} \right\} = \frac{e^2}{1-e^2} \sin^2 GM.$$

$$\text{Cor. } \tan^2 GN = \frac{\tan^2 b}{\cos^2 b \sin^2 a} (\cos^2 AG - \cos^2 a \cos^2 b).$$

March 27, 1873.

Sir GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

It was proposed by the Earl of Rosse, and seconded by Dr. Sharpey,—

That the best thanks of the Royal Society be given to Dr. Bence Jones for his munificent present of a bust of Faraday.

THE BAKERIAN LECTURE.—"On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its variation in amount with her Phases." By the EARL OF ROSSE, D.C.L., F.R.S., &c. Received February 8, 1873.

(Abstract.)

In this paper is given an account of a series of observations made in the Observatory of Birr Castle, in further prosecution of a shorter and less carefully conducted investigation, as regards many details, which forms the subject of two former communications* to the Royal Society.

The observations were first corrected for change of the moon's distance from the place of observation and change of phase during the continuance of each night's work, and thus a curve, whose ordinates represented the scale-readings (corrected) and whose abscissæ represented the corresponding altitudes, was obtained for each night's work. By combining

* Proceedings of the Royal Society, vol. xvii. p. 436, vol. xix. p. 9.

all these, a single curve and table for reducing all the observations to the same zenith-distance was obtained, which proved to be nearly, but not quite, the same as that found by Professor Seidel for the light of the stars. By employing the table thus deduced, and also reducing the heat-determinations obtained on the various nights for change of distance of the sun, a more accurate phase-curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Professor Zöllner's gives for the moon's light.

By employing Laplace's formula for the extinction of light in our atmosphere, the heat-effect in terms of the scale-readings was deduced, and an approximation to the height of the atmosphere attempted.

From a series of simultaneous measurements of the moon's heat and light at intervals during the partial eclipse of November 14, 1872, when clouds did not interfere, it was found that the heat and light diminish nearly if not quite proportionally, the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half what they were before and after contact with the penumbra.

April 3, 1873.

WILLIAM SHARPEY, M.D., Vice-President, in the Chair.

The following communications were read :—

- I. "On the Structure of Striped Muscular Fibre." By EDWARD ALBERT SCHÄFER. Communicated by Dr. SHARPEY, V.P.R.S.
Received March 13, 1873.

(Abstract.)

After premising that, owing to the rapidity with which changes set in after death, the subject in question can only properly be worked out whilst the muscular fibres are still living, the author proceeds to give the result of his investigations of the tissue in this condition. The animal employed was the common large water-beetle, the muscles of the legs being taken. These were examined entirely without addition, being either teased out upon a glass slide in the ordinary way and covered with thin glass, or else prepared upon the latter, which was then inverted over a ring of putty after the method introduced by Stricker.

The author describes a muscular fibre as consisting of a ground-substance appearing at first sight to be formed of two distinct substances (the one dim, the other bright in aspect, which are arranged in alternating disks disposed in successive series, with their planes at right angles to

the axis of the fibre) and of a vast number of minute rod-like particles, to which he applies the term *muscle-rods*, which are closely arranged side by side and parallel to the axis of the fibre, so as to form by their juxtaposition as many series as there are disks of dim substance in the fibre. The main part or shaft of each muscle-rod is imbedded in and traverses a disk of dim substance, while the ends, which are enlarged at the extremity into little knobs or heads, extend into the bright disks. These little knobs it is which give the appearance of the line of dots which has long been described as existing in the middle of each bright stripe; when the fibre is somewhat extended this line appears double, owing to the separation of the heads of the two successive series of muscle-rods which meet in the middle of the disk of bright substance. The author describes the rods as differing somewhat both in relative position and in form, these differences being accompanied by corresponding changes in the appearance of the ground-substance. The principal changes are those of form. Thus, in what the author is inclined to regard as the state of absolute rest, the rods are uniformly cylindrical without terminal enlargements; in this case only a longitudinal fibrillation is to be seen in the fibre, all trace of transverse striping having disappeared. In the normal state of slight tension, however, the rod-heads make their appearance, and with them the bright substance by which they are surrounded, so that the dim ground-substance now presents a transversely striated aspect. In contraction of the muscle the heads of the rods become enlarged at the expense of the shaft, the extremities of each muscle-rod thus approaching one another: the enlarged heads, being closely applied both to the neighbouring ones of the same series and to those of the next series which meet them in the bright stripe, the line of dots now appears as a dark transverse band with bright borders. As the contraction proceeds, and these dark bands approach one another, the bright borders encroach upon the dim stripe, which finally disappears, so that its place is taken up by a single transverse bright stripe. Consequently contracted muscle shows alternate dark and light stripes; the former, however, are in this case due to the enlarged juxtaposed extremities of the rods, the light on the other hand being mainly composed of the ground-substance which has become accumulated in the intervals between their shafts.

After giving a description of the appearances observed in transverse section, when examined in the normal state without addition, and after the consideration of those which are met with in sections from frozen muscle examined in $\frac{1}{2}$ per cent. solution of common salt, and which have been described by Cohnheim, the author proceeds to consider the nature of the ground-substance, and more especially the transversely striated appearance which it ordinarily presents. He gives it as his opinion that the ground-substance is in reality uniform in nature throughout, and that the bright bands which cross it are due to an optical effect produced

by the presence of the globular heads of the muscle-rods, which have a different refractive index from that of the ground-substance. That such an explanation is possible, is shown by the examination of minute oil-globules imbedded in gelatine, which appear under the microscope as dark spots with a bright surrounding, the juxtaposition of several such dots giving the effect of a bright band. That the bright transverse bands in muscle are similarly produced by the juxtaposition of the rod-heads would appear from the following amongst other considerations:—

1. Where the rod-heads are smaller the bright bands are correspondingly narrower.
2. Where the rod-heads have become merged into the shafts, so as no longer to be seen as distinct objects, the bright transverse stripes have also entirely disappeared.
3. When in contraction the rod-heads enlarge and encroach on the shaft, their bright borders accompany them and encroach on the dim substance, so that at last all appearance of dimness becomes entirely obliterated, the bright borders becoming blended in the middle.
4. The part of the muscle-rod where the head joins the shaft is rendered indistinct by the brightness around the rod-head; whereas if this brightness were inherent in the ground-substance, this part of the rod would stand out all the darker by the contrast.
5. The appearance of a transverse section is corroborated; for in this case the rod-heads are seen so close together that the optical effect of any one would become merged into those of its neighbours: consequently the whole of the intermediate substance would appear bright; and this is actually found to be the case.
6. The fact that both the dim and the bright substance of resting muscle appear doubly refracting would seem to indicate that they are of the same nature.

The author then proceeds to give the result of his investigations of the appearance of muscle under polarized light. He finds that, as regards muscle at rest when placed between crossed Nicols, the whole fibre appears illuminated; in the contracted state, on the other hand, the appearance is presented of illuminated stripes with dark intervals. The latter correspond with the lines formed by the juxtaposition of the enlarged ends of the muscle-rods; these consequently are singly refracting (isotropic), and so, in all probability, are the shafts of the muscle-rods; they do not, however, stand out as *black* streaks, since they are surrounded by doubly refracting (anisotropic) ground-substance, and are illuminated by the light which has previously traversed this. In the same way it may readily be understood why, in the resting muscle also, the rods, although isotropic, do not appear as such. The conclusion, then, that the author arrives at on this point is that the whole of the muscular fibre is anisotropic, with the exception of the muscle-rods.

Various observers are then quoted in support of the accuracy of the description given; and the probability is pointed out, and supported by an observation of Prof. Brücke, that in all cases in which alternating disks of isotropous and anisotropous substances are observed, the muscular fibre is in a state of contraction (although not necessarily shortened)—that is to say that the anisotropous substance has become accumulated between the shafts of the rods, the isotropous disks being due to the rod-heads, between which there is no perceptible amount of anisotropous substance left remaining.

The author concludes the paper by offering a conjecture as to the nature of the substances which, according to his description, compose the proper substance of muscle, and as to the probable mode in which the contraction is effected. He is inclined to regard the intermediate ground-substance as the true contractile part, and thinks that it may be allied in nature to ordinary protoplasm, the rods, on the other hand, being elastic structures, and merely serving to restore the fibre to its original length.

II. "Note on the Synthesis of Marsh-gas and Formic Acid, and on the Electric Decomposition of Carbonic Oxide." By Sir B. C. BRODIE, Bart., D.C.L., F.R.S., late Waynflete Professor of Chemistry in the University of Oxford. Received April 3*, 1873.

In connexion with the investigation on the electric decomposition of carbonic-acid gas referred to in a previous communication to the Society, I was led to submit a mixture of hydrogen and carbonic-oxide gas to the action of electricity in the induction-tube, the mixed gases being circulated through the tube by means of an apparatus which I will not now describe. A contraction was soon observed to have taken place, which at the end of an hour amounted to 10 cub. centims. The rate of contraction steadily diminished, and during the fifth hour of the duration of the experiment amounted to only 2 cub. centims. The experiment was stopped, and the gas analyzed with the following results in two several analyses:—

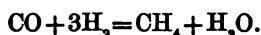
I.		II.	
Carbonic oxide	61·65	Carbonic oxide	61·35
Hydrogen	32·16	Hydrogen	32·34
Marsh-gas	6·14	Marsh-gas	6·31
	<hr/> 100·00		<hr/> 100·00

A small quantity (about 2 per cent.) of nitrogen was also contained in

* Received in nearly the same form March 27.

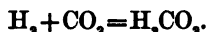
the gas, together with a trace of oxygen, which have been omitted from the calculation.

The result of this reaction is expressed in the following equation :—



This fundamental experiment, which constitutes the basis of a new method of chemical synthesis, susceptible of the most varied applications, and of peculiar interest in reference to the explication of natural phenomena, was commenced by me on the 10th of January last at Oxford, in the laboratory of my friend and successor in the Chair of Chemistry, Professor Odling; and two analyses of the gas were completed, and the results attained in the course of a week from that date.

In a similar experiment made with a mixture of hydrogen and carbonic-acid gas, a contraction also occurred, attended with the formation of water. The gas which resulted from the experiment was found to consist (after the absorption of carbonic acid) of hydrogen and carbonic oxide, together with a little marsh-gas. Traces of oxygen and nitrogen were also present. Minute drops, too, of an oily liquid appeared in the tube. This liquid, after the conclusion of the experiment, was dissolved in a small quantity of water. The solution was strongly acid and had a pungent taste. It reduced an alkaline solution of terchloride of gold and an ammoniacal solution of nitrate of silver. These reactions are the characteristic properties of formic acid, of which we may infer the synthesis to have been effected according to the equation



I may avail myself of the present opportunity to place on record the following important facts in reference to the action of electricity on carbonic-oxide gas.

When pure and dry carbonic oxide is circulated through the induction-tube, and there submitted to the action of electricity, a decomposition of the gas occurs, attended with a gradual and regular contraction, which, in the form assumed in my experiments, occurred at the regular rate of about 5 cub. centims. in an hour. Carbonic acid is formed, and simultaneously with its formation a solid deposit may be observed in the induction-tube. This deposit appears as a transparent film of a red-brown colour, lining the walls of the tube. It is perfectly soluble in water, which is strongly coloured by it. The solution has an intensely acid reaction.

The solid deposit in the tube, in the dry condition before it has been in contact with water, is an oxide of carbon. Samples, however, made in different experiments do not present precisely the same composition; but nevertheless they appear to belong to a certain limited number of forms which repeatedly occur, and may invariably be referred to the same general order or system. This system is, or appears to be, what I may term an

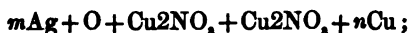
homologous series of "oxycarbons," of which the unit of carbon with the weight 12 may be regarded as the first term, and of which the adjacent terms differ by an increment of carbonic oxide (CO) weighing 28, precisely as homologous series of hydrocarbons differ by the increment CH_2 with the weight 14. I have succeeded in identifying by analysis two at least of these substances, namely the adjacent terms C_4O_2 and C_5O_4 . From this point of view these peculiar bodies are members of a series of oxycarbons analogous in the oxycarbon system to the series of hydrocarbons of which the unit of carbon is the first and the unit of acetylene C_2H_2 is the second term, the oxycarbon C_4O_2 being represented in that series by the hydrocarbon crotonylene C_4H_6 , and the oxycarbon C_5O_4 by the hydrocarbon valerylene C_5H_8 .

III. "On an Air-Battery." By J. H. GLADSTONE, Ph.D., F.R.S., and ALFRED TRIBE, F.C.S. Received March 6, 1873.

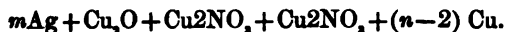
The galvanic battery which we are about to describe is founded on a reaction that we brought under the notice of the Royal Society last spring*. We then showed that if pieces of copper and silver in contact are immersed in a solution of nitrate of copper in the presence of oxygen, a decomposition of the salt ensues, with the formation of cuprous oxide on the silver and a corresponding solution of the copper, while a galvanic current passes through the liquid from copper to silver. We stated, moreover, that this was no isolated phenomenon, but only one of a large class of similar reactions. It seemed desirable to examine more fully the history and the capabilities of the electrical power thus produced.

It was previously ascertained that the combination of the oxygen takes place only in the neighbourhood of the silver; and the following formulæ may serve to render the chemical change and transference more intelligible:—

Before contact,



after contact,



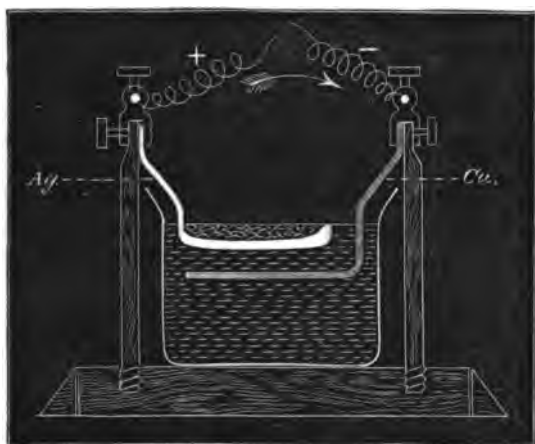
This action is evidently a continuous one until either the oxygen or the copper fails.

Now the oxygen of the atmosphere is practically unlimited in amount, but there is a difficulty in bringing any large quantity of it into contact at once with the silver and the dissolved salt.

To facilitate this, we arrange that the silver plate should have a horizontal position just under the surface of the liquid in the cell; and,

* Proc. Roy. Soc., April 1872, vol. xx. p. 290.

in fact, we convert it into a small silver tray full of crystals of the same metal which rise in projections to the very surface. The copper plate lies horizontally under it, separated, if need be, by a piece of muslin, and connexion is made by a wire as usual. The vertical part of the copper plate, from a little above the liquid to the bend, should be varnished; otherwise solution principally takes place there, which causes the horizontal part of the plate to drop off. Holes are made in the silver tray with the view of shortening the communication between the air-surface and the copper plate and of facilitating the movements of the salt in solution. The solution itself may be contained in a shallow trough or saucer, and the whole arrangement will be somewhat as in the sectional view here given :—



That dissolved oxygen is absolutely necessary for this chemical change has been already shown; but it was interesting to measure by a galvanometer the difference of the currents obtained by means of an ordinary, that is aerated, solution of copper nitrate, and one from which the air had been separated to the greatest possible extent. A Thomson's galvanometer was employed, which had a resistance of 2631.5 units at 18°·3 C. Two cells were prepared with vertical plates and alike in all respects, except that the one contained an ordinary 6 per cent. solution of copper nitrate, and the other a similar solution which had been deoxygenized by the means described in our former paper. Another experiment was made with a different pair of cells and an 11 per cent. solution. It was necessary to use the 1.99 shunt; and the following were the amounts of deflection :—

Time after immersion.	Expt. I.		Expt. II.	
	Oxygenized.	Deoxygenized.	Oxygenized.	Deoxygenized.
1 minute.	78	14	130	11
4 minutes.	72	9	90	8
12 „	68	6	75	6
49 „	—	—	58	3.5

The contrast is evident. That the deoxygenized solution does give a deflection at all is due partly to the difficulty of excluding air, and partly, perhaps, at first to the oxygen condensed on the surface of the silver plate. The effect due to the water itself is inappreciable.

From the nature of the reaction it might be expected that the current would gradually diminish on account of the using up of the dissolved oxygen in the neighbourhood of the silver; such a diminution always does take place, at least after the first few vibrations of the needle.

It might be expected, too, that when the amount of action has run down considerably, the mere moving of the liquid so as to bring fresh parts of the solution against the silver would augment the currents. It does so.

The same might be predicted from stirring up the crystals of silver in the tray so as to expose new surfaces. This also was found to be the case.

And, again, it might be anticipated that if the wire were disconnected for a time so as to allow the oxygen to diffuse itself from other parts of the solution, and the connexion were made, the current would be found as strong, or nearly so, as before. That also is true in fact.

A cell with the plates connected by a wire was placed under a bell-jar full of air over mercury. The mercury gradually rose inside, as might be expected from the absorption of the oxygen in the air.

The necessity of oxygen and the avidity with which it is taken up are both illustrated by the following experiment:—Two cells with horizontal plates were prepared alike in every respect, except that the first was filled with a solution simply deprived of oxygen, the second with a solution through which a stream of carbonic-acid gas had been passed for some time. The first was placed in the air, the second in a vessel from which the air had been expelled by allowing carbonic-acid gas to flow into it for an hour or two.

The deflections obtained were as follows, the 1-999 shunt being used and the temperature being 13°.7 C:—

Time after immersion.	In air.	In CO ₂ .
1 minute.	165	76
5 minutes.	135	82
10 „	135	58

As the cell in an atmosphere of carbonic-acid gas showed considerable action, in fact nearly half as much as that in the air, each cell was short circuited for 23 hours, with the expectation that any oxygen in the closed vessel would be used up; and, indeed, the most prominent crystals of silver in the cell in carbonic-acid gas became reddened, while a cuprous deposit extended over the whole of the crystals in the other cell. When, however, the short wires were removed and the galvanometer interposed, the cell in the air gave a deflection of 136, practically the same as before, but that in carbonic-acid gas, instead of showing a great decrease, rose to 80. It was then found that the vessel containing the latter slowly admitted air; so the contents were swept out by a fresh stream of carbonic-acid gas, and it was made properly air-tight. After connexion by a short wire for 3 days the galvanometer indicated a deflection of 20, that of the cell in the air being 110, temperature 10° C. As this showed a very great reduction of the chemical action, carbonic-acid gas was again passed through the vessel for an hour or two; and after a connexion of two more days the indication of the galvanometer was only 3, while the other cell gave 115, the temperature being now 10°·5 C. The action, therefore, was at last reduced almost to nothing; and the original fault in the experiment brought out, perhaps more clearly than would otherwise be seen, how eagerly the solution will absorb even minute quantities of oxygen from the surrounding gas.

An important point to determine was the best strength of the copper nitrate solution. Six per cent. was generally preferred, for two reasons:—first, it gives about the maximum of effect—a solution four times as strong gives less than half the deflection, and a solution only a quarter as strong gives only two thirds; secondly, a stronger solution than this 6 per cent. is apt to produce a deposit, not of pure cuprous oxide, but of a subnitrate, which was supposed to clog up the silver crystals to a greater extent.

Another point investigated was the best proportion between the areas of the metallic surfaces. Experiments were made with vertical plates, in which the silver was kept at a uniform size and the copper was diminished by covering it more and more with varnish; and another set was made in which the copper remained the same, while the silver plate was reduced.

The results may be thus exhibited:—

Proportion of surfaces.		Deflection.		
Silver.	Copper.	Expt. I.	Expt. II.	Expt. III.
1	0.25	24	23	..
1	0.5	28	27	..
1	0.75	31	30	..
1	1.0	33	32	28
1	1.33	28
1	2.0	32
1	4.0	30

The increase of the copper surface, therefore, has comparatively little effect.

Proportion of surfaces.		Deflection.		
Copper.	Silver.	Expt. I.	Expt. II.	Expt. III.
1	0.25	7.5
1	0.5	16
1	0.75	21
1	1.0	33	32	28
1	1.33	41	40	..
1	2.0	56	54	..
1	4.0	96	92	..

The increase, therefore, of the silver or negative metal causes an almost proportionate increase in the chemical action. This, doubtless, arises from the necessity of oxygen, and explains the value of the large surface exposed by the silver crystals in the tray.

The effect of heat on the action of this cell was examined; it increases the action greatly: thus an arrangement which gave a deflection of 40 at 20° C. gave one of 250 at 50° C.; and the augmentation was observed to be much more rapid in the higher than in the lower portions of this range of temperature.

If the formula given above for the reaction be a true one, it follows that every atom of copper deposited on the silver in the state of suboxide must be compensated by an atom of copper dissolved from the copper plate. This was proved quantitatively. In a cell that had been in action for a week the loss of the copper plate was 0.391 grm., while

the suboxide deposited on the silver was found to be equivalent to 0.398 grm. of metallic copper. This deposit of suboxide, though it soon forms apparently a complete covering to the silver, does not greatly diminish the action; it is probably porous, besides being itself a conductor of electricity. In some cases we have found it deposited in crystals sufficiently large to be seen by the naked eye, and which are shown by a magnifying-glass to be regular octahedra.

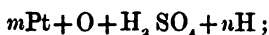
The internal resistance of this battery, as might be expected, is small.

The electrolytic power of the current was examined. One cell, the plates of which were about two inches in diameter, was found sufficient to decompose such metallic salts as the nitrates of copper, silver, or lead, copper sulphate or stannous chloride, in aqueous solution, when platinum was used for the negative electrode, and for the positive the same metal as existed in the salt experimented on. Six cells were sufficient to decompose dilute sulphuric acid slowly and dilute hydrochloric acid pretty quickly, copper electrodes being employed.

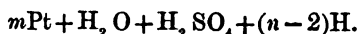
The theoretical interest of this battery lies mainly in the fact that it differs essentially from every other galvanic arrangement, inasmuch as the binary compound in solution is incapable of being decomposed either by the positive metal alone or by the two metals in conjunction; it cannot serve, in fact, as the liquid element of the circuit without the presence of another body ready to combine with one of its constituents when set free.

Grove's gas-battery is essentially different from ours if the oxygen and hydrogen condensed on the platinum plates play the part of the two metals; but it closely resembles ours if hydrogen acts the part of the positive and platinum that of the negative metal; the dilute sulphuric acid, a hydrogen compound, will then be decomposed on account of the simultaneous presence of the oxygen, which can combine with the liberated hydrogen. Viewed in this manner Grove's gas-battery is only a special case of the general reaction which we have described in our previous paper; and the formula will be:—

Before contact,



after contact,



The practical interest of our arrangement lies in the fact that it is an approximation towards a constant air-battery. Should it ever come into use elsewhere than on the lecture-table, it will probably be in the form of a combination of zinc and copper, with an aerated solution of zinc chloride; for that arrangement has an electromotive force six times that of the arrangement we have more particularly studied, and about three quarters that of a Daniell's cell. The numbers representing the difference of potential between the two metals, which were actually

obtained by means of an electrometer belonging to Sir William Thomson, were:—

Silver and copper with deoxygenized copper nitrate	4
Ditto „ oxygenized ditto	8 to 11
Copper and zinc with chloride of zinc	62
Ditto „ water	68
Ditto „ Daniell's cell	83

Chloride of zinc is preferred to the sulphate, as it offers less internal resistance, and a solution of 20 per cent. is recommended as about the best conductor*. A single cell of this description is capable of decomposing dilute sulphuric or hydrochloric acid, when copper electrodes are employed. The two metals might be arranged as in a Daniell's battery; the zinc would of course require no amalgamation, and the whole might be left for weeks or months without any attention. The oxide of zinc produced generally falls to the bottom of the vessel, and may be separated whenever it is thought desirable.

The power is thus obtained at a minimum of expense, for the oxygen which combines with the zinc costs nothing. Such a battery would appear to be specially adapted to cases where the galvanic current has to be frequently broken, as in telegraphy; for at each period of rest it renews its strength by the absorption or diffusion of more oxygen from the air.

The Society then adjourned over the Easter Recess, to Thursday, April 24th.

Presents received March 6, 1873.

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* On the authority of Mr. Herbert MacLeod.

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corrosion of the Menai Bridge. The author on that occasion visited the bridge, and found that no corrosion had taken place, careful attention to proper means of prevention having been sufficient to preserve the structure. The proved value of periodical inspections in the case of steam-boilers is then alluded to, and the author urges the importance of similar inspections of iron ships by the Admiralty and by private shipowners.

As respects riveted joints, the author first discusses the relative merits of machine- and hand-riveting, and points out that in machine-riveting there is much greater security that the rivets properly fill the rivet-holes. The rate at which riveting can be done is given as 16 rivets per minute with a lever riveting-machine, 10 rivets per minute with a steam riveting-machine, and 0.66 rivet per minute when the riveting is done by hand.

Experiments on Riveted Joints.

The author alludes, first, to the series of experiments on riveted joints which were communicated by him to the Royal Society in 1850. Those experiments showed that, taking the strength of a solid plate at 100, the strength of an equal width of riveted joint would be 70 if the joint were double riveted, and 56 if the joint were single riveted: in other words, 30 per cent. of strength is lost in a double-riveted joint, and 44 per cent. in a single-riveted joint. Since that time it has appeared to some engineers that the process of punching was injurious to the plates, and in many cases recourse has been had to drilling to form the rivet-holes. On this point the author was of opinion that with good iron no sensible injury resulted from punching the rivet-holes; on the other hand, with rigid and bad plates, the plate would crack between the rivet-holes, and the plate would have to be discarded. He regarded it as a distinct advantage of the punching-process that it thus tested the quality of the iron. It had been found in certain cases that the rivets sheared across with a less force when in drilled than when in punched holes, and this had been attributed to the sharpness of the edges of the drilled holes. This point seemed to require investigation, and experiments to elucidate it had, therefore, been made, the results of which are narrated in the paper. The theory that the sharp edges of drilled holes induced a cutting action, which diminished the strength of the rivets, had suggested to Colonel Inglis, R.E., to try whether a greater resistance in the joint could be obtained by purposely rounding the edges of the rivet-holes. Some experiments had been made for Colonel Inglis by Mr. Kirkaldy, from which it appeared that the resistance of the rivet was increased 10 per cent. by this rounding of the edges of the rivet-holes. The author has also experimented on this point, and the results of his experiments are given in the paper.

A series of tables of experiments are then given, each result being fully discussed. The following Table is a summary of results obtained by the author.

Experiments on the Ultimate Resistance of Rivets to Shearing.

No. of experiments.	Diameter of rivet, in decimals of an inch.	Sectional area of rivet, in decimals of a square inch.	Shearing resistance of rivet, in tons.	Shearing resistance, in tons per square inch.	Elongation of joint, in dec. of an inch.	Remarks.
1.	·84	·554	10·819	19·528	·190	Single shear; punched holes; machine-riveted.
2.	·82	·528	10·879	20·606	·272	Single shear; punched holes; hand-riveted.
3.	·84	·554	10·257	18·514	·168	Single shear; drilled holes; machine-riveted.
4.	·82	·528	10·739	20·339	·190	Single shear; drilled holes; hand-riveted.
5.	·84	·554	11·221	20·254	·270	Single shear; drilled; edges of plates rounded; machine-riveted.
6.	·82	·528	11·192	21·197	·240	Single shear; punched and countersunk; hand-riveted.
7.	·84	·554	10·850	19·584	·160	Single shear; drilled and countersunk; machine-riveted.
8.	·82	·528	11·234	21·276	·288	Single shear; drilled; edges rounded; countersunk; hand-riveted.
9.	·82	·528	23·375	44·270	·340	Double shear; punched; hand-riveted.
10.	·82	·528	20·684	39·174	·232	Double shear; drilled; hand-riveted.
11.	·82	·528	24·313	46·047	·482	Double shear; drilled; edges rounded; hand-riveted.
12.	·82	·528	26·464	50·121	∞	Double shear; drilled; edges rounded and countersunk; hand-riveted. (This rivet did not shear. The plate broke away.)

The general conclusions drawn by the author from his experiments are as follows:—

(1) *Joints with drilled holes are weaker and elongate less before fracture than joints with punched holes.* The average of four experiments on joints with drilled holes, compared with the average of four experiments on joints with punched holes, shows that the rivets in the former are 1·36 ton per square inch, or $6\frac{1}{2}$ per cent., weaker than the rivets in the latter. The elongation is also 26 per cent. less with drilled than with punched holes.

(2) *Hand-riveted joints are somewhat stronger than machine-riveted joints.* The mean of three experiments on hand-riveted joints, compared with the mean of three experiments on machine-riveted joints, shows an excess of shearing resistance in the rivets in the hand-riveted joints amounting to 1·495 ton per square inch, or $7\frac{1}{2}$ per cent. The hand-riveted joint also elongates 26 per cent. more than the machine-riveted joint.

(3) *There is a decided increase in the strength of the rivet when the edges*

of the rivet-holes are rounded, so as to diminish their cutting action. The mean of three experiments on joints with rounded holes, compared with the mean of three experiments on joints with unrounded punched holes, and with the mean of three experiments on joints with unrounded drilled holes, gives the following results:—

	Shearing resistance of rivet.		
Rounded holes.....	21·517 tons per sq. in.		
Unrounded holes { punched	20·953	”	”
drilled	19·228	”	”

This shows that the joint with rounded holes is, as regards the resistance of the rivet, 12 per cent. stronger than the joint with drilled holes, but only $2\frac{3}{4}$ per cent. stronger than the joint with punched holes.

As regards the apparent superiority of hand-riveting as compared with machine-riveting, the author observes that this is due to the fact that in hand-riveted joints the rivet is slightly hardened by being hammered after it is cold. In small experimental joints hand-riveting has therefore a slight advantage in strength; but the defects of hand-riveting are not likely to be exhibited in experiments on a small scale.

The experiments seem conclusive as to the inferiority of joints with drilled holes; and the author reiterates his conviction that the force used in punching plates is a valuable practical test of their quality, and tends to prevent the use of inferior iron.

APPENDIX I.

In this Appendix are given the results of Colonel Inglis's experiments on joints with rivet-holes the edges of which are rounded. These experiments agree with the author's in most respects; they show the superiority of punched to drilled holes, but the difference of the strength of the rivets in Colonel Inglis's experiments is only $2\frac{1}{2}$ per cent. On the other hand, in Colonel Inglis's experiments the superiority of the joints with rounded holes is much greater than in the author's, amounting on the average to 29 per cent.

APPENDIX II.

On the Theory of the Resistance of Riveted Joints.

In this Appendix the ordinary theory of the resistance of riveted joints is discussed, and some points in which it is defective are pointed out. It is shown that a riveted joint may give way either:—(1) by the tearing of the plates from the rivet-hole to the edge of the plates; (2) by the tearing of the plates from rivet-hole to rivet-hole; (3) by the shearing of the rivet. When the plate gives way by tearing from the rivet-hole to the edge of the plate, a bending stress is induced in the

part of the plate in front of the rivet. The amount of that bending stress is approximately estimated, and a rule found for the distance of the rivet-hole from the edge of the plate. When the plate gives way by tearing from rivet-hole to rivet-hole, it is commonly assumed that the stress on the part of the plate between the rivets is a uniformly distributed stress. This is shown to be not strictly correct, and the want of uniformity of stress will cause the plate to give way with a lower average intensity of stress than that which corresponds to the ultimate resistance of the plate to tension. How much the plate may be weakened by the want of uniformity in the distribution of the stress it is impossible to calculate. Probably the loss of strength due to this cause is in ductile plates very small; but it is pointed out that this weakening may account, at least in part, for the apparent loss of strength of the plates at joints as compared with the same plates broken in an unperforated condition. This loss of strength has been hitherto ascribed entirely to injury done to the plate in the punching-process. When the rivet gives way by shearing, the stress on the section is also not uniform. In consequence of the great deformation of the rivet before fracture, it is subjected to bending as well as shearing action. The friction between the plates induced by the contraction of the rivets in cooling has been supposed sometimes to add to the apparent resistance of the rivet to shearing. It is shown that a considerable displacement of the plates takes place before ultimate fracture, and that the deformation of the rivets is so great that it can hardly be supposed that they exert any tension, holding the plates together at the moment of fracture. The friction should therefore be entirely neglected in estimating the ultimate resistance of riveted joints; and this, indeed, has been done by most English writers.

The question of the resistance to deformation is then discussed, and a limit fixed for the safe intensity of the pressure on the bearing surface of the rivet.

The practical considerations affecting the diameter of the rivet are then stated; and joining these to the considerations given above, a series of rules for proportioning riveted joints are drawn up, and some Tables of the proportions of rivets and joints are given.

II. "On the Employment of Meteorological Statistics in determining the best Course for a Ship whose Sailing Qualities are known." By FRANCIS GALTON, F.R.S. Received March 13, 1878.

If we desire to estimate which of two alternative passages between the same ports would be performed most quickly on the average of many voyages, no knowledge can be more immediately useful than that of the distance which the ship could accomplish at various points of the routes

in a unit of time. The intention of the present memoir is to show how this desideratum may be most readily obtained, and the precise method by which, when it has been obtained, it should be turned to account. It should be added that in the earlier part of it I am obliged to recapitulate views which I have already published in the Transactions of the British Association, 1866 (Transactions of Sections), p. 17.

Suppose the meteorological statistics of some ocean district to show that, on the average, out of every 100 ships that visited it the weather recorded in the following Table was experienced :—

TABLE I. Statistics of weather.

30 ships find the wind N., with an average force of 3							
25	"	"	"	E.,	"	"	2
15	"	"	"	S.,	"	"	1
10	"	"	"	W.,	"	"	2
20	"	"	"	calm,	"	"	0
<hr/>							
100							

At first I will suppose no current to exist. I have grouped the winds under the 4 cardinal points for the sake of simplicity in explanation ; but it must be recollected that in practice they would be grouped under at least 8 points, and probably 16.

Let the sailing qualities of the ship be those specified in Table II., in which the figures have been extracted from an elaborate but, I fear, only approximate schedule of the performances of the standard ship of meteorologists, commonly described as the "Beaufort Ship." I have

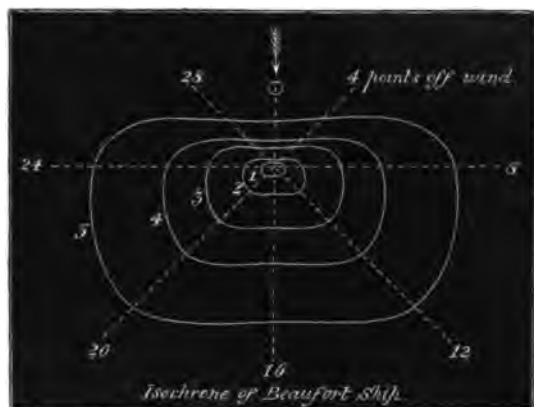
TABLE II. Sailing qualities of Ship.

Force of wind.	Number of miles made good in one day's sail.			
	Number of points at which the course of the ship lies off the wind, reckoning to the right.			
	0 points, or wind right ahead.	8 points, or wind abeam (same value as 24 points).	16 points, or wind astern.	24 points, or wind abeam (same value as 8 points).
1	5	12	12	12
2	10	38	34	38
3	24	89	79	89
4	38	139	125	139
5	62	230	202	230

protracted the data contained in the original schedule, and formed from them the curves represented in the annexed diagram (fig. 1).

From the materials contained in Tables I. and II. we have to calcu-

Fig. 1.



late the average distance towards each of the 4 cardinal points that the ship is capable of accomplishing in a day.

The ship that on each of the 100 occasions makes the best of her way to the N. experiences the following weather. The wind is right ahead of her (that is, is N.) on 30 occasions, with an average force of 3; therefore, under those circumstances, as we learn from Table II., she will sail an aggregate distance of $30 \times 24 = 720$ miles. On 25 occasions the wind is abeam (E.) of her, with a force of 2, and she sails under these conditions $25 \times 38 = 950$ miles; on 15 occasions it is astern (S.), with force 1, by which she makes $15 \times 12 = 180$ miles; and, lastly, it is abeam (W.) on 10 occasions, with force 2, contributing 380 miles. Therefore the total distance she would sail in the 100 voyages, each of one day's duration, and in every case to the N., is $720 + 950 + 180 + 380 = 2230$ miles; and consequently her average daily performance to that point of the compass is 22.30 miles.

Similar computations give 34.40 for an E. course, 37.75 for a S., and 38.00 for one to the W. The form of calculation is appended in Table III.

If we take a point A, fig. 2, and mark from it the distances we have just obtained, and draw a contour, with a free hand, enclosing the dots, we shall have a figure such as is represented, which determines the performance of the ship from A to every point of the compass. It should be borne in mind that, although there must be much guess work in drawing the curve under the guidance of only 4 dots, there will be considerably less chance of error when we have 8, and none of appreciable

Fig. 2.



amount when we have 16. In calculating to only 4 points, we have, as is shown in Table III., no more than 4 lines and 4 columns, or $4^2=16$ entries. If we dealt with 8 points, the number of entries would amount to $8^2=64$, and if with 16, to $16^2=256$ entries; but the amount of labour involved in such tedious computations need excite no apprehension, because I shall show how calculation may be entirely dispensed with, and the results obtained by the aid of a machine with remarkable facility and quickness.

TABLE III.

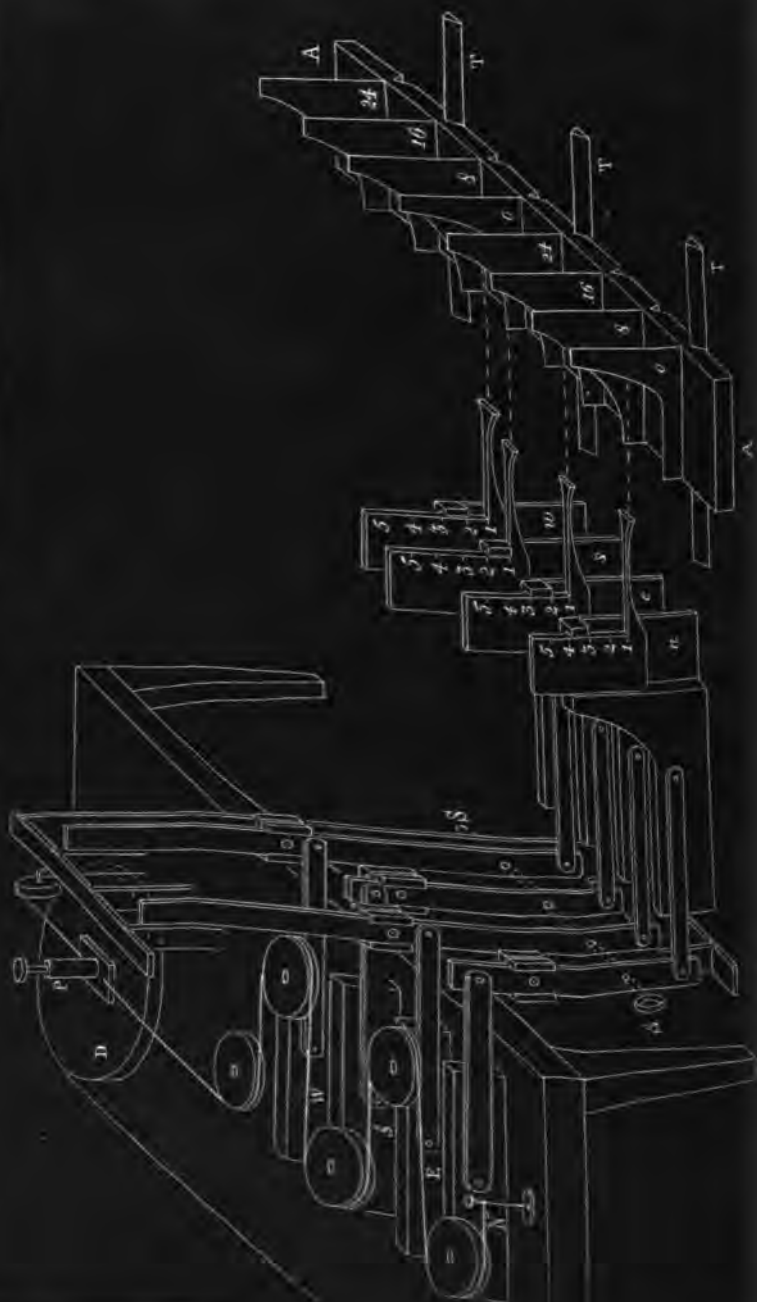
Course of ship.	Relative frequency and force of wind.				Total miles on each course.
	N. 30 per cent. Force 3.	E. 25 per cent. Force 2.	S. 15 per cent. Force 1.	W. 10 per cent. Force 2.	
N.	Wind ahead, 0 point. 720 miles.	Abeam, 8 950	Astern, 16 180	Abeam, 24 380	2230
E.	Abeam, 24 2670	Ahead, 0 250	Abeam, 8 180	Astern, 16 340	3440
S.	Astern, 16 2370	Abeam, 24 950	Ahead, 0 75	Abeam, 8 380	3775
W.	Abeam, 8 2670	Astern, 16 850	Abeam, 24 180	Ahead, 0 100	3800

We have thus far supposed no current to exist; if there should be a current, the above contour would be incorrect as regards the position of A; but it would be perfectly correct in regard to the position in which a float would be found at the end of the day which had been dropped in the water at the beginning, because the float and the ship would have drifted simultaneously in parallel lines. If, then, A be the point of departure and A' be the position of the float at the end of the day, and if we draw a contour, as described above, round A', it will be true for the joint effects of winds and currents for A. Conversely, in order to construct a contour for their joint effects from A, we have first to draw A A' to represent the drift of the current in one day from A, and then to use A' as a point of construction for a contour calculated upon the data of the winds alone; the contour so drawn will be the required figure, with A for the point of departure.

It seems strange that so useful and definite a conception as that of the contour we have been considering should not long since have acquired a name. The idea in its general application is not unfamiliar to us, because we are all accustomed to reflect how far we can travel in an hour's walk, ride, or drive from our house, in various directions, and we know that the distances in different directions vary according to the goodness or straightness of the road. Nevertheless our vocabulary does not admit of an expression of the idea upon which this memoir is based, without a tedious paraphrase including references to (1) equal and specified times, (2) direct geographical distance, (3) universality of directions, and (4) a bounding line. I therefore propose to employ the word *isochrone* (equality of time) in a special sense in this memoir, meaning thereby all that is expressed by the contour just described, which I should call the "isochrone from A," the unit of measurement being understood to be a single day's sail, unless otherwise specified. The isochrone *towards* A will be the same figure reversed and inverted.

The lines of swiftest passage from one port to another can only be determined after computation of the isochrones for a sufficient number of ocean districts within the region of inquiry to enable those at any particular spot, or as much of them as is needed, to be found by interpolation. It seems to me perfectly impossible to draw any portion of an isochrone, except in a rudely approximative way, without previous computation. Calculation to 8 or 16 points takes so long to perform, and small differences in the mean force of the winds have so large an influence, that no human brain is competent to deduce correct results after a mere inspection of the data. As an example, I may be allowed to mention that I asked a naval officer of unusually large experience in the construction of weather charts, and who was familiar with the sailing qualities of a "Beaufort standard ship," to estimate portions of isochrones in certain cases; and I found the mean error of his estimates to exceed 15 per cent. The guesses of ordinary navigators would necessarily be much more wide of the truth. Now we must recollect that a very small saving on the average length of voyages would amount to an enormous aggregate of commercial gain, and that, where precision is practicable, we should never rest satisfied with the rule of thumb. Our meteorological statistics afford the best information attainable at the present moment, and they exceed by some hundredfold the experiences of any one navigator; their probable errors may nevertheless be large, but that is no reason for needlessly associating them with additional subjects of doubt. The probable error of a navigator's estimate of an isochrone, and consequently of the data which he must use, whether consciously or not, whenever he attempts to calculate his best track, is due at the present time to no less than three distinct sets of uncertainties:—*a*, the average weather; *b*, the performance of his ship on different courses with winds of different force (which I understand to be hardly ever ascertained with much precision); *c*, the computa-

Fig. 3.



tion of the isochrone. If A, B, and C be the probable errors of these three respectively, then the probable error of the navigator's estimate will be $\sqrt{A^2+B^2+C^2}$. My desire is to reduce this large item to a simple A, which may itself be minimized until it ceases to be of any practical inconvenience. It is no new thing that statistics should require discussion and elaborate calculation before they can be turned to account and be made the familiar basis of vast commercial undertakings. Classified lists of ages at death are no more fitted to be the immediate guides of those who grant annuities or engage to pay reversions, than are the crude meteorological statistics of the ocean to be the immediate guides of the navigator.

It is probable that most vessels may practically admit of division into some moderate number of classes, and that it would suffice to calculate isochrones for each of these classes; but in any case the number of calculations must be very large, because they would differ not only for the class of ship and the particular destination, but also for the season of the year when the voyage was made. It is therefore important that even individual ships should be enabled to have isochrones drawn for their especial use at a trifling cost. I will now show how this may be effected by mechanical means.

The drawing I give (fig. 3) is only a diagram to explain the principle of the machine; the framework is left out, and the proportions are somewhat varied for the convenience of illustration. Also, for simplicity of explanation, I have supposed the machine constructed to apply to no more than 4 points of the compass, and it is represented as adjusted to work out the example already given in Table III.

A long tray, open at the top and almost wholly open along the front, has 8 grooves, into which pieces of zinc, thin wood, or even stout card-board may be dropped, much like the glass plates in a photographer's box. In fig. 3 we only see the base of this tray, A A, and the pieces of zinc standing upon it in the position in which they would be held by the grooves. The zinc plates have curved edges in front, which refer to the sailing qualities of the particular ship under consideration; these are cut out from the data in Table II., each plate corresponding to the column whose heading it bears. The ordinate of the curve is proportionate to the force of the wind, and the abscissa to the distance sailed in one day on the specified course (0, 8, 16, or 24) with that force of wind. There are grooves cut in A A, one under each zinc plate, and there are tramways, T, upon which A A may be set, in gear with those grooves. In the figure it is so set that 0 is opposite to *n*, 8 to *e*, 16 to *s*, and 24 to *w*; but if it were lifted up and laid one groove more to the left, 0 (on the right hand) would be opposite to *w*, 8 to *n*, 16 to *e*, and 24 to *s*. Similarly, by setting it two or three grooves to the left, the other possible variations would be gone through. The slides *n*, *e*, *s*, and *w* refer to the course of the ship

shown in the first column of Table III., and the four different settings correspond to the four lines in the body of that Table.

The slides *n*, *e*, *s*, and *w* move to and fro parallel to the tramways, and are each furnished with bars that can be pushed vertically up and down, and a rod projects horizontally from each of the bars towards the zinc plate opposite to it. Graduations referring to the force of the wind are placed on the bars, which are thereby adjusted to come in contact at the proper levels with the zinc plates, and to be pushed back through a distance equal to the length of the abscissæ at those levels, when *A A* is run forwards on its tramway.

Thus far we have obtained the result that *n*, *e*, *s*, and *w* shall be severally pushed back to the distances which would be sailed over in one day if the wind blew on 100 occasions with specified forces from *each* of those quarters, and if the ship were sailing to that quarter whose initial is opposite to the zinc plate marked 0. As *A A* is adjusted in the figure, that quarter would be *N.*; if it were moved one step to the left it would be *W.*, if 2 steps it would be *S.*, and if 3 steps it would be *E.*

We have now to diminish the movements impressed upon *n*, *e*, *s*, and *w* in the ratio of the percentage of occurrence of the several winds. This is effected by linking them to another series of slides, *N*, *E*, *S*, and *W*, as shown in the figure, and by attaching adjustable centres to the arms, there shown in a vertical position. The standards to which those centres would be clamped are almost wholly removed in the figure, but the top and bottom of them can be seen. The reduction will be correct within such limits as we need, when the links are somewhat longer than in the figure and the arm does not swing through more than 40°. I therefore do not care to propose in this case the somewhat more complicated, but perfectly accurate arrangement which I contrived for the parallel slides of the pantagraphs now in use at the Meteorological Office, of which a description is given in the Report of the Meteorological Committee for 1870, p. 30.

I place the adjustable centres between the links in the cases of *N* and *S*, and above them in those of *E* and *W*; consequently, when *n*, *e*, *s*, and *w* are all pushed back, *N* and *S* will advance, and *E* and *W* will retreat: the reason for doing this will be seen presently. In order to graduate the arms for the adjustable centres, we must take the $\frac{1}{100}$ part of the distance between the pivots of the links as the unit of measurement, and measure in both cases from the pivot of the upper link as the zero-point. Then if *p* = the percentage to which the movements are to be reduced, the graduations for *p* between the links are determined by the formula $\frac{100p}{100+p}$, and when above the links by $\frac{100p}{100-p}$.

The results we have now attained are that, when *A A* is pushed forward, *N*, *E*, *S*, *W* shall move alternately forwards and backwards, each through a distance corresponding to that which a ship would sail towards the

quarter whose initial is opposite to the zinc plate marked 0, under the several influences of the N., E., S., and W. winds as they are found to occur. What remains is to sum up these movements.

I put a pulley, running easily on its axis, upon each of the upper slides, as in the figure, and pass a band, one of whose ends is secured to a fixed peg, round these pulleys, alternately over and under them. The free end of the band is kept stretched by a light weight, and a framework, carrying a vertical pricker, is urged to and fro by the movements of the band. A disk (D), upon which the drawing-paper is secured, has its centre exactly below the pricker when the machine is at zero; and whenever A is pushed home, the pricker travels in a radial distance to an amount equal to twice the sum of the movements of the several slides, and therefore through a distance proportionate to a day's sail of the ship towards the quarter whose initial letter is opposite to the zinc plate marked 0. If desired, the numerical value of the movement of the string could of course be read off.

The manipulation of the instrument would be as follows:—

1. Remove A A.
2. Push or pull the slides *n, e, s, w* into their mean position.
3. Thrust a skewer, S S, through the holes in the arms and framework to hold every thing fast.
4. Adjust the centres, and clamp them if necessary.
5. Adjust the bars for force of wind.
6. Remove the skewer.
7. Push the slides *n, e, s, w* as far back as they will go.
8. Replace A A, and pull the slides forward to it. The machine is now in working order.
- a.* Push A A home.
- b.* Press the pricker, not forgetting to mark the N point.
- c.* Turn the disk through a quadrant.
- d.* Pull back A A, and set it one step in advance on the tramways.
- e.* Pull the slides up to it.

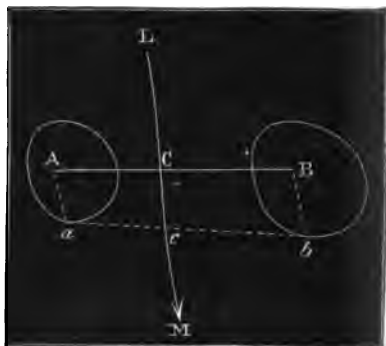
The series 1 to 8 has to be gone through once for all for each isochrone; that from *a* to *e* for each of the 4 points of the compass. The whole of these actions are simple and rapid, and the adjustments are of the easiest kind. An isochrone based upon 4 points ought to be leisurely plotted out in $2\frac{1}{2}$ minutes, and one based upon 8 points in 4 minutes.

This step-by-step arrangement is far easier of construction than one, which may suggest itself to many persons, in which the movement should be continuous, using a curved surface instead of a set of curved edges, and by which the entire curve should be drawn instead of a few points pricked out; also it is far more convenient and compact not to arrange the machine in a circular form, which is that which would most naturally first be thought of.

When the isochrones have been drawn to scale on a chart, isochronic lines at various points along any proposed route could readily be found by graphical interpolation. Thus, in fig. 4, let the route be from L to M, and let the isochrones round A and B be known. Draw A *a*, B *b* parallel to L M, and join A B, *a b*, cutting L M in C and *c*; then C *c* is one day's sail from C. We can do more than this; for we may find the distance of

a day's sail from *any* point of LM by the following device. Erect Cc perpendicular to LM , and equal to Cc ; similarly, erect other perpendiculars ($L'l$, $M'm'$) from points L , M , &c., of which the day's sail along the route has been laid down in the same way that Cc was, and draw with a free hand a line through l' , c' , m' ; then the perpendicular distance Xx' from any point X on the line LM to its intersection at x' with the line l' , c' , m' will be the length of a day's sail from X , and it can be transferred with a pair of compasses to find x on the line L , X , M .

Fig. 4.

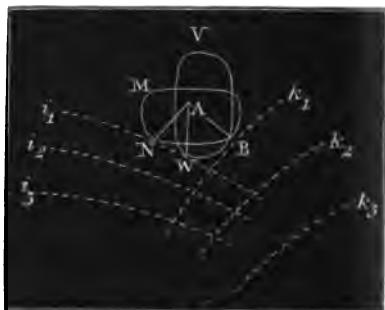


Another application of the principle of isochrones is to draw them at distances of 1, 2, 3, &c. days for ships bound *towards*, not from, a given port. The first day's isochrone requires no explanation. For that of the second day, a few neighbouring points must be selected on the contour already drawn, and from these such small portions as are needed of other isochrones must be constructed; the line sketched with a free hand to bound these figures will be the isochrone of two days' journey towards the port, and similarly for the third and subsequent days. The lines of shortest passage from curve to curve join the points whence the subsidiary isochrones were drawn with the points where the latter are in contact with their bounding line. The appearance of a chart so constructed would be that of a series of roughly concentric curves round the port and of radial lines of shortest passage. I give no illustration of such a chart, because it seemed a waste of labour to calculate one upon the scanty data available at the present moment, when far more ample and trustworthy materials have been collected and are in the course of gradual publication by the Meteorological Office; and calculations would have been necessary, because the machine just described exists only in the form of a rude model.

We have, lastly, to explain how the navigator would use one of these charts on specific occasions. Suppose (fig. 5) he finds himself at A with such and such weather, present and probable, how should he steer?

He must use an isochrone appro-

Fig. 5.

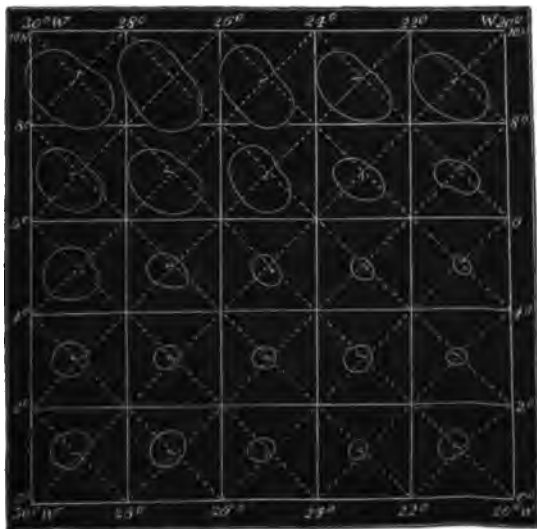


priate to the occasion, most likely out of a stock which he would keep ready to hand ; and applying it to his map, he would note the direction in which it will cut the largest number of isochrones towards the port. If we take cases where the isochrones for the occasion are (1) N M B and (2) W V B, the first being with a N. and the second with a W. wind, then, if the isochrones towards the port be as in the series k_1, k_2 , &c., the course to be steered in both instances is A B ; but if they are as in the series i_1, i_2 , &c., the course in case (1) will be A N, and in case (2) A W, quite independently of the direction in which the port may happen to lie.

There is much to say about the proper method of discussing the crude statistics derived from ocean districts artificially bounded by lines of latitude and longitude in order to obtain the most probable meteorological values, but I will only allude to them here. First, homogeneous districts and periods of time have to be made out ; secondly, the crude observations in each subdivision of those districts have to be discussed in connexion with those made at adjacent subdivisions in the same district ; and, thirdly, they have to be discussed in reference to those made in preceding and succeeding periods of time. There is no doubt but that labour spent in these discussions would after a time become more remunerative than the same amount of labour in accumulating fresh observations.

I submit (fig. 6) a series of 8-hour isochrones computed to 8 points from the

Fig. 6.



crude observations taken in each "2-degree square" of the ocean between

10° north latitude and the equator, and between 20° and 30° west longitude, in the month of January. The uniformity of their sequence is very striking; and it would no doubt have been still more so if the data had previously been discussed in the above-mentioned manner. The short line with an arrow-head shows the direction and amount of current; but the centre of each square is the point of departure, for which the contour shows the joint effects of winds and current.

To recapitulate. I have shown in this memoir :—1, what isochrones are, and their great importance; 2, how to calculate them; 3, how to construct a machine to supersede their calculation and to make it possible to have them drawn for special cases at a trifling cost; 4, how to make an isochronal chart; and, 5, how to use it on individual occasions.

I should be glad if one result of this memoir were to bring into greater prominence than at present the high value of the ocean statistics collected and now being published by the Meteorological Office, and the fact that no degree of precision of meteorological knowledge need be thrown away in the practice of navigation. Such knowledge will be good for all time, and will always afford the requisite data whence isochrones conformable to the varying performances of new varieties of ships and to new lines of commerce may be calculated.

May 1, 1873.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows :—

William Aitken, M.D.	James Augustus Grant, Lieut.-Col.,
Sir Alexander Armstrong, M.D.,	C.B.
K.C.B.	Clements Robert Markham, C.B.
Robert Stawell Ball, LL.D.	George Edward Paget, M.D., D.C.L.
John Beddoe, M.D.	George West Royston-Pigott, M.D.
Frederick Joseph Bramwell, C.E.	Osbert Salvin, M.A.
Edward Killwick Calver, Capt.	The Hon. John William Strutt, M.A.
R.N.	Henry Woodward, F.G.S.
Robert Lewis John Ellery, F.R.A.S.	James Young, F.C.S.

Dr. Arthur Gamgee was admitted into the Society.

The following communications were read :—

I. "On the Condensation of a Mixture of Air and Steam upon Cold Surfaces." By OSBORNE REYNOLDS, M.A., Fellow of Queen's College, Cambridge, and Professor of Engineering in the Owens College, Manchester. Communicated by Professor ROSCOE, F.R.S. Received March 22, 1873.

1. The object of this investigation is to ascertain how far the presence of a small quantity of air affects the power of a cold surface to condense steam. *A priori* it seemed probable that it might retard condensation very much; for when pure steam comes up to a cold surface and is condensed, it leaves an empty space which is immediately filled with fresh steam; so that the passage of the steam up to the cold surface is unobstructed, and if the surface could carry off the heat fast enough, then the rate of condensation would be unlimited. If, however, the steam is mixed with air, then, as the mixture comes into contact with the cold surface, the steam will be condensed and the air will be left between the fresh steam and the cold surface; so that after condensation has commenced that surface will be protected by a stratum of air, and fresh steam will have either to displace this or pass through it before it in turn can be condensed.

2. This question, besides its philosophical interest, has important practical bearings on the steam-engine.

First. If the quantity of air mixed with the steam affects the rate at which it condenses, then the ratio which the pressure of air bears to the pressure of steam in a condenser will materially affect its efficiency: this is particularly important with reference to the *surface-condenser*.

Second. If air prevents the condensation of steam, then by sending air into the boiler of a *high-pressure engine*, the condensation at the surface of the cylinder will be prevented, which, if allowed to occur, becomes a source of great waste; for when the steam comes into a cold cylinder it condenses, heating the cylinder and leaving water, which will again be evaporated as soon as the steam escapes; and this, in evaporating, will cool the cylinder. By preventing this, the mixing of air with the steam would effect the same object as the steam-jacket, only in a more efficient manner; for the heat communicated to the steam in the cylinder from the jacket is not nearly so effective as that which is communicated from the boiler, in consequence of the steam in the cylinder being at a lower temperature than that in the boiler.

3. The experiments for this investigation were, by the kind permission of Dr. Roscoe, carried out by Mr. Pasley, a student in the Chemical Laboratory of the Owens College; and I beg to tender him my best thanks.

4. In making these experiments two objects were particularly kept in view:—

First. To ascertain if there is a great difference in the rate of condensation of pure steam and a mixture of steam and air—to ascertain in fact whether pure steam condenses at an unlimited speed.

Second. To ascertain if (and according to what law) the effect of air on the condensation increases as the proportion of air to steam increases.

5. Of these two undertakings the first is much the most difficult. The rate of condensation of pure steam is so great that it is practically impossible to measure it; and to institute a comparison between this and the condensation of a mixture of steam and air is like comparing the infinite with the finite. It is practically impossible to keep any surface cold when an unlimited supply of pure steam is condensed upon it, so that under such circumstances the quantity of pure steam condensed is limited by the power of the surface to carry off the heat. The best method of obtaining a qualitative result seems to be by introducing sufficient cold water into a flask of steam to condense it all, and ascertain whether this condensation is effected suddenly or slowly.

6. The presence of hot water in the flask with the steam very much assists in ascertaining the rapidity of condensation. When there is no hot water in the flask, the condensation by the injected water is only a question of time; the gauge will come to the same point whether the condensation is quick or slow, the only difference being in the speed at which it will rise—a difference not easy to appreciate, especially when the motion is quick. But if hot water is present, then as the steam in the flask is condensed it is replaced by fresh steam from the water, and the interval between the condensation and the consequent ebullition is the only time allowed for the creation of a vacuum; the vacuum which is attained in the interval will therefore depend on the rapidity of condensation. The interval will be very short; and the better the vacuum the shorter it will be; so that unless the condensation is very sudden, there will be but a slight reduction of pressure.

If, however, the condensation is really instantaneous, a perfect vacuum may exist for an instant. Hence, when there is water in the flask, the rapidity of condensation is indicated by the height to which the gauge rises, instead of the speed with which it rises; and this is much easier to estimate.

7. The apparatus employed in making these experiments consisted of a glass flask fitted with a mercurial vacuum-gauge and pipes for admitting water and air, or allowing steam to escape.

The flask and all the pipes were freed from air by boiling; and when all the air had been driven out the pipes were closed, the lamp removed, and the flask allowed to cool until the gauge showed a slight vacuum; the water-pipe was then opened and a few drops of water allowed to enter and fall through the flask; as they did so the mercury rushed up the gauge, and, by its momentum, above the point for a perfect vacuum, showing that the condensation was instantaneous. Immediately afterwards the gauge fell nearly to its starting-point. Next, the flask was allowed to cool and a little air was let in (about equal to half an inch of mercury in the gauge, or about a sixtieth of the volume of the flask). The lamp was then replaced, and the operation was repeated as before;

this time, however, as the cold water entered the mercury did not rush up the gauge, but rose slowly a small distance and there remained.

8. This experiment shows, therefore, that there is a great difference in the rates at which pure steam and steam with air condense on a cold surface, so great in fact that the speed with pure steam must be regarded as nearly infinite.

9. *To compare the various effects of different quantities of air, two methods have been used*, which may be described as follows:—

I. A surface-condenser is formed within the boiler or flask, so that the steam may be condensed as fast as it is generated. Then, when a flame of a certain size acts on the boiler, the effect of the air is to cause the pressure of steam in the flask to increase. This method is founded on the assumption that the rate at which steam will condense at a cold surface is, *ceteris paribus*, proportional to its pressure—an assumption which is probably not far from the truth.

II. With the same apparatus as in method I. the rate of condensation is measured by the quantity of water condensed in a given time, obtained by counting the drops from the condenser, the pressure within the flask being kept constant. This method does not involve any assumption; but the conditions for its being accurate are such as cannot be obtained; for not only must the temperature of the condenser and the temperature of the steam remain constant, but the pressure of the steam must also remain constant, and if the two former conditions are fulfilled the latter cannot be; for the temperature of the steam will be the boiling-point of the water in the flask; and if this is to remain constant, the pressure of air and steam must be constant, and therefore, as the pressure of the air increases, the pressure of the steam must decrease. This variation of pressure is not very great; and its effect may be allowed for on the assumption that the condensation is proportional to the pressure of steam. This is accomplished by dividing the drops by the pressure of the steam.

These methods, neither of which, as it appears, is rigorous, seem nevertheless to be the best; and fortunately the law which the effect of the additions of air follows is of such a decided character as to be easily distinguished; and the two methods give results which are sufficiently concordant for practical purposes.

10. The apparatus employed in these experiments consisted of a glass flask, in which a surface-condenser was formed of a copper pipe passing in and out through the cork. This pipe was kept cool by a stream of water, and was so fixed that all the condensed water dropped from it, and the drops could be counted. The flask was freed from air by boiling; the volume of air passed into the flask could be accurately measured; and ample time was allowed for the air in the flask to produce its effect before more was admitted.

For the experiments according to method I., the flame under the flask and the stream of water through the condenser were kept constant from first to last. For those made according to method II., in one case the

stream of water was kept constant, and in the other it was altered, so that the effluent water was kept at a constant temperature.

11. The results of these experiments are shown in Tables I., II., III.

The letters which head the columns have the following meanings:—

f stands for the volume of the flask in cubic centimetres.

a stands for the volume of the air at the pressure of the atmosphere.

h_0 stands for the height of the barometer in millimetres of mercury at the time of the experiment.

h_1 stands for the height of mercury in the gauge in millims.

t_0 stands for the temperature Centigrade of the effluent water.

t_1 stands for the temperature of the water in the flask, found from Regnault's tables of boiling-points.

$p_1 = h_0 - h_1$ stands for the pressure within the flask in millims. of mercury.

$p_2 = \frac{a}{f} + h_0 \frac{t_1 + 274}{t_0 + 274}$ stands for the pressure of the air within the flask corrected to the temperature T_1 .

$p_3 = p_1 - p_2$ stands for the pressure of the steam.

$\frac{p_2}{p_3}$ stands for the ratio of the pressure of the air in the flask to that of the steam.

TABLE I.

$h_0 = 756$, $t_0 = 9$, $f = 500$.

a .	t_1 .	h_1 .	Drops per minute.	p_1 .	p_2 .	p_3 .	$\frac{1}{p_3}$.	$\frac{p_2}{p_3}$.	$\frac{2000}{p_3}$.
0	9	754	...	2	0	2	0	...
0	22	736	...	20	0	20	0500	0	100
1.5	36	712	...	44	2.4	41	0240	06	48
5.0	52	654	...	106	8.7	97	0110	09	22
10	66	557	56	199	18.4	183	0055	10	11
13	70	521	...	235	23.7	211	0050	11	10
21	77	433	...	323	40.0	283	0035	14	7
30	84	330	...	426	57	368	0027	15	5.4
40	88	264	...	492	77	414	0024	18	4.8
50	93	179	56	577	97	479	0020	20	4
60	96	115	...	641	117	523	0019	22	3.8
70	98	55	...	701	138	562	0017	24	3.4
80	100	0	...	756	159	596	0016	26	3.2

TABLE II.

$h_0 = 457$, $f_0 = 500$.

a .	t_0 .	t_1 .	h_1 .	Drops per minute.	p_1 .	p_2 .	p_3 .	$\frac{p_2}{p_3}$.	$\frac{\text{Drops}}{p_3}$.	$\frac{\text{Drops}}{p_3}$.
0	27	66	567	100	190	0	190	53	42.4
2.5	24	"	572	84	185	4.5	180	022	45	36
5	20	"	582	59	175	9	166	055	35	27
10	13	"	582	21	175	19	156	12	14	11.2
27	10	"	582	10	175	48	127	39	76	6.0
37	10	"	579	10	177	66	111	66	9	7.2
50	9	"	572	8	184	90	94	10	8	6.4

TABLE III.

$$h_0 = 748, \quad t_0 = 11, \quad f = 500.$$

$a.$	$t_1.$	$h_1.$	Drops per minute.	$p_1.$	$p_2.$	$p_3.$	$\frac{p_2}{p_3}.$	$\frac{\text{Drops}}{p_3}.$
0	6	741	..	7	0	7	0	0
...	47	663	106	85	0	85	0	125
3.2	66	557	106	191	6	185	0.32	60
5	"	"	56	"	9	182	0.50	30
10	"	"	21	"	18	173	1.04	11
15	"	"	17	"	27	164	1.63	10
20	"	"	12	"	36	155	2.3	8
30	"	552	10	196	54	142	3.9	7
40	"	557	8	191	72	119	6.0	6½
50	"	562	7	186	90	96	9.3	7

12. Table I. shows the result of an experiment after the first method, during which the flame and condensation remained constant, whilst the pressure within the flask increased with the quantity of air.

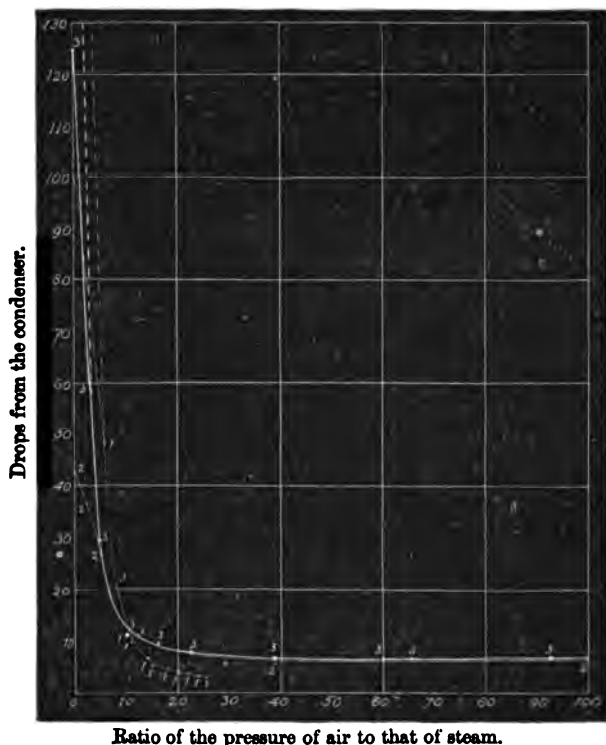
Table II. shows the result of an experiment after the second method, in which the pressure within the flask remained constant, whilst the flame and condensation were reduced as the air was admitted. In this experiment the rate at which the water passed through the condenser was constant from first to last, and consequently the temperature of the effluent water varied with the condensation.

Table III. shows the result of an experiment, also made according to the second method, but in which the quantity of water flowing through the condenser was so varied that the temperature of the effluent water remained constant.

13. Each of these Tables shows the effect of air on the condensation in a very definite manner; but the results as given in the column p_2 in Table I. cannot be compared with the $\frac{\text{Drops}}{p_2}$ in Tables II. and III. as they stand; for these show the effect of the air in a series of increasing figures. If, however, these figures show the power of the air to diminish condensation, then they will be inversely proportional to the quantity of water condensed, i. e. what would have been condensed if the pressure and other things had remained constant. Hence the numbers in the column $\frac{1}{p_1}$ should be proportional to the numbers in the column $\frac{\text{Drops}}{p_2}$ in Tables II. and III.

In order to compare the results of these experiments, the results in each Table have been multiplied by a common factor, so that they may be the same when the pressure of air is one tenth that of the steam. Thus the numbers in the column $\frac{1}{p_1}$ in Table I. have been multiplied by 2000, and numbers under $\frac{\text{Drops}}{p_2}$ in Table II. by 7. The result of the experiments thus reduced are shown in the curves 1, 2, 3.

The point of no air might have been chosen as the point in which the curves should coincide; but, as has been previously explained, the results under such circumstances are to be taken as indicating the power of the condenser to carry off the heat. Had it been possible to keep the condenser cool, then there is reason to believe that there would have been no limit to the condensation of pure steam, and that the true form of the curves is like that shown by the dots.



Although the curves do not coincide, yet they are all of the same form, and the difference between them is not greater than can be accounted for by the disturbing causes already mentioned. They all show that the effect of air begins to fall off rapidly when its pressure amounts to one-tenth that of the steam, and that when it amounts to about one fourth that of the steam the admission of more air produces scarcely any effect.

14. *Conclusions.*—The conclusions to be drawn from these experiments are as follows :—

1. That a small quantity of air in steam does very much retard its condensation upon a cold surface; that, in fact, there is no limit to the rate at which pure steam will condense but the power of the surface to carry off the heat.

2. That the rate of condensation diminishes rapidly and nearly

uniformly as the pressure of air increases from two to ten per cent. that of the steam, and then less and less rapidly until thirty per cent. is reached, after which the rate of condensation remains nearly constant.

3. That in consequence of this effect of air the necessary size of a surface-condenser for a steam-engine increases very rapidly with the quantity of air allowed to be present within it.

4. That by mixing air with the steam before it is used, the condensation at the surface of a cylinder may be greatly diminished, and consequently the efficiency of the engine increased.

5. That the maximum effect, or nearly so, will be obtained when the pressure of the air is one tenth that of the steam, or when about two cubic feet of air at the pressure of the atmosphere and the temperature 60° F. are mixed with each pound of steam.

15. *Remarks.* As this investigation was nearly completed my attention was called to a statement by Sir W. Armstrong, to the effect that Mr. Siemens had suggested as an explanation of the otherwise anomalous advantage of forcing air into the boiler of a steam-engine, that the air may prevent, in a great measure, the condensation at the surface of the cylinder. It would thus seem that Mr. Siemens has already suggested the probability of the fact which is proved in this investigation. I am not aware, however, that any previous experiments have been made on the subject, and therefore I offer these results as independent testimony of the correctness of Mr. Siemens's views as well as of my own.

II. "On the direct Synthesis of Ammonia." By W. F. DONKIN.

Communicated by Sir B. C. BRODIE, Bart., F.R.S. Received May 1, 1873.

The action of induced electricity on mixtures of certain gases has been lately shown by Sir Benjamin Brodie (Proc. Roy. Soc. April 3, 1873) to yield very interesting results.

An obvious application of his method was to treat a mixture of dry hydrogen and nitrogen in a similar manner as those referred to above, with the view of effecting the synthesis of ammonia; and Sir B. Brodie kindly allowed me the use of his apparatus for the purpose of the experiment, which was conducted as follows:—

A mixture of about three volumes of hydrogen with one of nitrogen in a bell-jar over water, was passed through two tubes containing pumice moistened with alkaline pyrogallate and sulphuric acid respectively, then through a Siemens induction-tube, and into a bulb containing dilute hydrochloric acid. The whole apparatus being first filled with pure hydrogen, about half a litre of the mixed gases was sent through the apparatus, the induction-coil not being in action; the bulb containing the acid was then removed and another substituted, containing an equal volume of the same acid.

About half a litre of the mixed gases was now passed through the apparatus, submitting them to the action of the electricity. The contents of the two bulbs were next transferred to two test-tubes; and after adding excess of potash to each, Nessler's test was applied. The first solution gave a faint yellow coloration, the second a rather thick reddish-brown precipitate.

No attempt was made to estimate the quantity of ammonia formed, as it would vary with many of the conditions of the experiment.

Since writing the account of the above experiment, which was made in Dr. Odling's laboratory at Oxford on March 24, I have seen in the '*Comptes Rendus*' for April 22, 1873, a note of an experiment by Messrs. Thénard of Paris, in which they observe the formation of traces of ammonia by the action of electricity on a mixture of hydrogen and nitrogen; but no details of the mode of operating are given.

III. "On the Effect of Pressure on the Character of the Spectra of Gases." By C. H. STEARN and G. H. LEE. Communicated by W. HUGGINS, F.R.S. Received March 19, 1873.

The variations in the spectra of gases which accompany changes of density have been studied by Plücker and Hittorf, Frankland and Lockyer, Wüllner and others.

It appears to us that one cause to which these changes may be due has been overlooked, and that many of the observed variations are entirely independent of the density of the gas. If a Leyden jar be placed in the circuit, and the current from an induction-coil be passed through a Plücker's tube containing nitrogen with the traces of hydrogen generally present, the following well-known phenomena are observed.

When the gas is near atmospheric pressure, the line-spectrum of nitrogen is brilliant, and the F line of hydrogen is broad and nebulous. As the pressure is reduced, the lines of nitrogen gradually fade out, and the band-spectrum appears, while at the same time the F line of hydrogen becomes narrow and well defined. If fresh gas be admitted, the line-spectrum reappears, accompanied by a widening of the F line.

That these changes are not dependent on the density of the gas, appears from the following experiment:—

A sealed tube containing nitrogen, with traces of hydrogen at a pressure of about 2 millims., was placed before the spectroscope. A second tube was connected with the air-pump, and the current passed through both tubes, a Leyden jar being placed in the circuit.

When the pressure in the second tube was high, the line-spectrum of nitrogen appeared brilliantly in the sealed tube, and the F line was broad and nebulous; as the exhaustion proceeded these lines faded out, and the F line became narrow, in precisely the same manner as if the sealed tube had been in process of exhaustion. The explanation appears to be that

the production of the line-spectrum of nitrogen, and the expansion of the F line of hydrogen, depend entirely on the intensity of the charge communicated to the Leyden jar. When the pressure of the gas between the electrodes is high, the discharge does not take place until the jar is fully charged; but as the exhaustion proceeds a less and less charge is communicated to the jar, and the discharge at last is virtually not more than that of the simple current.

The same effect may be produced by interposing a break in the circuit, the length of which may be increased as the pressure in the tube is reduced. Plücker and Hittorf appear to have used a break, as in their paper in the *Philosophical Transactions*, Nov. 1864, they speak of the expansion of lines obtained by increasing the charge of the jar by an interposed stratum of air. They do not, however, appear to have noticed that the reduction of pressure in the tube was only equivalent to a diminution of the charge of the jar, and that to this cause many of the changes of spectra which accompany the reduced pressure ought to be ascribed.

We are continuing our experiments on the effect of temperature on the spectrum, but prefer to reserve this portion of the subject for the present.

May 8, 1873.

FRANCIS SIBSON, M.D., Vice-President, in the Chair.

The following communications were read:—

- I. "The Action of Light on the Electrical Resistance of Selenium." By Lieut. SALE, R.E. Communicated by J. N. LOCKYER, F.R.S. Received March 28, 1873.

It having been recently brought to notice that selenium in the crystalline condition exhibits the remarkable property of having a conductivity varying with the degree of light to which it is exposed, the following experiments were undertaken with a view to the further elucidation of the matter:—

Experiment 1.—A bar of crystalline selenium measuring approximately $1.5'' \times .5'' \times .05''$ was procured, and platinum wire terminals were fastened to the ends.

The bar itself was then enclosed in a box having a draw-lid, so as to admit or exclude the light at pleasure.

Then, the lid of the box being on, the resistance of the selenium was measured by means of a high-resistance galvanometer and a Wheatstone's bridge, with dial-coils capable of measuring up to 10,000,000 ohms. The battery-power was 2 cells Daniell.

The measurement was made on a dull cloudy day, and in a room of equable temperature.

The resistance having been carefully balanced, the lid of the box was withdrawn, when the resistance of the selenium fell instantaneously and

considerably, as indicated by the rapid movement of the spot of light on the galvanometer-scale.

Experiment 2.—The transition from darkness to the light given by an ordinary gas-burner (conditions as before), caused a slight and barely perceptible fall in the resistance.

Experiment 3.—The bar of selenium was next tried in the solar spectrum on a very bright cloudless day (conditions as before), except that more battery-power was used (10 cells Daniell).

The diffused daylight could not be cut off; so the trial was made in the most shaded part of an ordinary room, the spectrum being superimposed on the ordinary diffused daylight.

The resistances were very carefully balanced in each case, with the following results:—

Resistance in darkness	330,000
" " violet	279,000
" " red	255,700
" " orange	277,000
" " green	278,000
" " blue and indigo	279,000
" " centre of red	255,000
Resistance just on the outside edge of red, red side	220,000
Resistance in dark rays clear of red	228,000
Resistance in diffused daylight only	270,000
Resistance taken in the dark immediately after exposure (resistance rising)	310,000

The indications were very clear; and the bar of selenium was so sensitive to the action of the spectrum, that a slight movement of the prism produced a corresponding movement in the spot of light on the galvanometer-scale.

It is to be noted that in this experiment the reflecting galvanometer was placed on a heavy masonry pillar insulated from the floor for observatory purposes, and that the battery, bar of selenium, and resistance-coils were in another room, being connected by long and carefully insulated leads with the galvanometer.

Experiment 4.—The diffused light was cut off as much as possible by screens; and the resistances were again balanced in the solar spectrum. Conditions as in the last case.

Resistance of selenium in red	240,000
" just outside red	240,700
" in blue	270,000
" in such diffused light as came through screens	290,000
Light cut off by lid of box (resistance rising)	310,000

Experiment 5.—The selenium was also exposed to the spectrum of the electric light in a darkened room.

The effect was feeble; but by using more battery-power in balancing, it was possible to measure the swing of the spot of light when the selenium was suddenly exposed to the action of the light of the spectrum.

The maximum effect was obtained in, or just at the edge of the red, the violet and blue rays producing scarcely any effect.

Experiment 6.—The selenium was exposed to the full sunlight; the resistance fell enormously and instantaneously, and on balancing it was found to be little more than half what it was in the darkness.

The following were the general results of the experiments:—

Results.—(1) That the resistance of selenium is largely affected by exposure to light.

(2) That this effect is not produced by the actinic rays, but is at a maximum at, or just outside the red rays, at a place nearly coincident with the locus of the maximum of the heat-rays.

(3) That the effect of varying resistances is certainly not due to any change of temperature in the bar of selenium.

(4) That the effect produced on exposure to light is sensibly instantaneous, but that, on cutting off the light, the return to the normal resistance is not so rapid.

It would seem that there exists a power in rays nearly coincident with the heat-rays of high intensity, of altering instantaneously and without change of temperature the molecular condition of this particular element.

II. "Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun."—No. II. By J. NORMAN LOCKYER, F.R.S. Received March 14, 1873.

(Abstract.)

The observations in this paper are a continuation of those referred to in the previous communication bearing the same title. They deal (1) with the spectra of chemical compounds, and (2) with the spectra of mechanical mixtures.

I. *Chemical Compounds.*

Several series of salts were observed; these series may be divided into two:—1st, those in which the atomic weights varied in each series; 2nd, those in which the associated elements varied in each series. The following salts were mapped:—

Pb F₂, Pb Cl₂, Pb Br₂, Pb I₂; Sr F₂, Sr Cl₂, Sr Br₂, Sr I₂; Ba F₂, Ba Cl₂, Ba Br₂, Ba I₂; Mg F₂, Mg Cl₂, Mg Br₂, Mg I₂; Na F, Na Cl, Na Br, Na I.

The conditions of the experiments are described. The same aluminium cups, described in the first paper, were used; and the poles were arranged

in such a manner that they could at will be surrounded with any gas or vapour. Hydrogen was used in some of these experiments; it was purified in the usual manner by drying and freeing from traces of sulphuretted hydrogen; it was then passed over clean cut pieces of sodium, and admitted to the poles. An induction-spark from 5 one-pint Grove cells was used, the circuit being without the Leyden jar.

The lead compounds behaved (in air) as follows:—

The fluoride gave the eleven longest lines of the metal; but four were very faint.

The chloride gave nine lines; one of these was very short.

The bromide gave six lines; but one was a mere dot on the pole.

The iodide gave four lines distinctly and two as dots, one of which was scarcely visible.

It is pointed out that the decrease in length and number of lines follows the increase in the atomic weight of the non-metallic element, the lines dying out in the order of their length.

Barium was next experimented on, the same series of salts being used. A marked departure from the results obtained in the case of the lead compounds was observed, especially in the case of the fluoride, its spectrum being much the simplest; in fact it consisted of only 4 lines. Strontium behaved like barium; and so did magnesium fluoride. This anomalous behaviour was found to be most probably due to the exceedingly refractory nature of these fluorides, all of them being quite infusible, and non-volatile in any spark that was used.

Sodic fluoride, sodic chloride, sodic bromide, and sodic iodide exhibited a behaviour exactly the reverse of that of lead; i. e. the iodide showed most of the metallic spectrum.

The difference between flame-spectra and those produced by a weak electric discharge are then discussed. Beads of the chlorides &c. were heated in a Bunsen gas-flame; BaI_2 gave a "structure" spectrum (since proved to be due to the oxide) and the line at wave-length 5534.5, by very far the longest metallic line of barium. The bromide behaved like the iodide; and so did the chloride, except that its spectrum was more brilliant. Baric fluoride gave scarcely a trace of a spectrum, the oxide structure being scarcely visible, and 5534.5 very faint indeed. The strontium salts follow those of barium—4607.5, the longest strontium line, appearing in conjunction with an oxide spectrum. The strontic fluoride, however, refused to give any spectrum whatever. These results are compared with those obtained with the weak spark, and it is shown that the difference is one of degree: e. g. baric bromide gives 25 lines in the spark; these are the longest lines. In the flame it gives but one line; but this is the longest of all the barium lines, and indeed very far exceeds all the others in length. When the flame-spectra are compared with those produced by the low-tension spark, the spectra of the metals in the combination are in the former case invariably more simple than in the latter, so that only the very longest line or lines are left.

Some experiments made by Mr. R. J. Friswell to determine the cause of the similarity of the spectra of the various salts of the same metal observed in air are then given, the conclusion being that the spectrum observed is really that of the oxide.

Kirchhoff and Bunsen's, Mitscherlich's, and Clifton and Roscoe's prior conclusions on the points investigated are stated at length; and it is shown that the observations recorded, taken in conjunction with the determination of the long and short lines of metallic vapours, are in favour of the views advanced by Mitscherlich, Clifton, and Roscoe. For while the spectra of the iodides, bromides, &c. of any element in air are the same, as stated by Kirchhoff and Bunsen, the fact that this is *not* the spectrum of the metal is established by the other fact, *that only the very longest lines of the metal are present, increased dissociation bringing in the other metallic lines in the order of their length.*

The spectra have been mapped with the salts in hydrogen: here the spectra are different, as stated by Mitscherlich; and *the metallic lines are represented according to the volatility of the compound, only the very longest lines being visible in the case of the least-volatile one.*

The following are the conclusions arrived at:—

1. A compound body has as definite a spectrum as a simple one; but while the spectrum of the latter consists of lines, the number and thickness of some of which increase with molecular approach, the spectrum of a compound consists in the main of channelled spaces and bands, which increase in like manner. In short, the molecules of a simple body and of a compound one are affected in the same manner by their approach or recess, so far as their spectra are concerned; *in other words, both spectra have their long and short lines or bands.* In each case the greatest simplicity of the spectrum depends upon the greatest separation of molecules, and the greatest complexity (a continuous spectrum) upon their nearest approach.

2. The heat required to act upon a compound, so as to render its spectrum visible, dissociates the compound according to its volatility: the number of true metallic lines which thus appear is a measure of the dissociation; and doubtless as the metal lines increase in number the compound bands thin out.

Mitscherlich's observations, that the metalloids show the same structural spectra as the compound bodies, is then referred to, and the question is asked whether the molecules of a metalloid do not in structure lie between those of elements on the one hand and of compounds on the other.

These considerations are applied to solar and stellar spectra; the general appearance of the solar spectrum shows that in all probability there are no compounds in the sun.

Secchi's maps of a large number of stellar spectra are referred to as now indicating beyond all doubt the existence of compound vapours in

the atmosphere of some stars ; and it is suggested that the phenomena of variable stars may be due to a delicate state of equilibrium in the temperature of a star, which now produces the great absorption of the compound and now that of the elemental molecules.

II. *Mechanical Mixtures.*

The second part of the paper deals with the mechanical mixtures. Maps of the spectra of alloys of the following percentages are given :—

Sn and Cd	percentages of Cd	10.0, 5.0, 1.0, 0.15.
Pb and Zn	„ „ Zn	10.0, 5.0, 1.0, 0.1.
Pb and Mg	„ „ Mg	10.0, 1.0, 0.1, 0.01.

It is pointed out that the lines disappear from the spectrum as the percentage becomes less, the shortest lines disappearing first—and that although we have here the foreshadowing of a quantitative spectrum-analysis, the method is so rough as to be inapplicable.

It is then stated that further researches on a method which promises much greater accuracy are in progress.

The bearing of these results on our knowledge of the reversing layer of the sun's atmosphere is then discussed.

III. "Contributions to the Study of the Errant Annelides of the Older Palæozoic Rocks." By H. ALLEYNE NICHOLSON, M.D., D.Sc., M.A., Ph.D., F.R.S.E., Professor of Natural History in University College, Toronto. Communicated by Professor RAMSAY, F.R.S., Director-General of the Geological Survey of the United Kingdom. Received December 30, 1872.

(Abstract.)

In this communication the author endeavours to elucidate the abundant and obscure organic remains which are found so commonly in the Palæozoic rocks, and especially in the Silurian strata of Britain, and which are generally known by the vague and convenient names of "Fucoids," "Annelide-burrows," and "tracks." After expressing his opinion that the first step towards the study of these obscure fossils lies in the provisional grouping and naming of the more marked forms which are already known to exist, the author proceeds to divide the remains under consideration into two great groups. In the first of these groups are those fossils which are truly the *burrows* of marine worms, as distinguished from mere trails and surface-tracks. Some of these burrows (*Scolithus*) are more or less nearly vertical in direction as regards the strata in which they are found ; and they are to be looked upon as being true burrows of habitation. In this section are placed the genera *Scolithus*, *Arenicolites*, and *Histioderma*. Other burrows are of a totally

of different nature from the preceding, and may reasonably be compared to the burrows of the recent lobworms. These burrows run more or less horizontally as compared with the laminae of deposition, or they penetrate the strata obliquely. They are not burrows of habitation, but are wandering tunnels excavated by the worm in its search after food. The fossils of this group, therefore, as preserved to us, are not the actual burrows themselves, but the burrows filled up with the sand or mud which the worm has passed through its alimentary canal. The burrows of this kind (including many forms previously described under the names of *Chondrites*, *Palæophycus*, &c.), the author groups together under the name of *Planolites*.

The second great group of Annelide-remains comprises genuine surface-trails or "tracks," which of necessity never pass below the surface of the bed on which they occur. Some of these remains, such as *Crossopodia*, are, beyond doubt, due to the operation of marine Annelides; but it may be a matter of question whether we have in these cases the actually petrified body of the worm, or merely the track produced by the passage of the animal over the surface of the mud or sand. The author, however, gives reasons for believing that the latter explanation is truly the correct one. Other fossils belonging to this group (such as *Myrianites*) are equally, beyond doubt, produced by the operations of marine animals; but it remains quite uncertain whether they have been formed by Annelides, Crustaceans, or Mollusks. Lastly, there are remains which appear to be really casts of the surface-trails of Annelides or other marine creatures, and which, therefore, are elevated above the surface of the bed on which they occur. Such remains may readily be confounded with those belonging to the genus *Planolites*, from which they are only distinguishable by the fact that they are strictly confined to a single surface of deposition. To fossils of this nature the author proposes to restrict the generic title of *Nemertites*.

Finally, the author describes some singular tracks apparently produced by Crustaceans belonging to the genus *Ceratiocaris*, and for which he proposes the generic name of *Caridolites*.

The following list comprises the species of fossils described in this communication :—

A. BURROWS.

I. Genus *ARENICOLITES*, Salter.

1. *Arenicolites sparsus*, Salter.
2. ——— *didymus*, Salter.
3. ——— *robustus*, Nicholson.

II. Genus *SCOLITHUS*, Haldemand.

4. *Scolithus canadensis*, Billings.
5. ——— *linearis*, Hall.
6. ——— *verticalis*, Hall.

III. Genus HISTIODERMA, Kinahan.

7. *Histioderma hibernicum*, Kinahan.

IV. Genus PLANOLITES, Nicholson.

8. *Planolites vulgaris*, Nicholson.

9. — *granosus*, Nicholson.

10. — *articulatus*, Nicholson.

B. TRAILS.

V. Genus CROSSOPODIA, M'Coy.

11. *Crossopodia scotica*, M'Coy.

12. — *lata*, M'Coy.

VI. Genus NEMERTITES, M'Leay.

13. *Nemertites Ollivantii*, Murchison.

14. — (*Palæochorda*) *major*, M'Coy.

15. — (*Palæochorda*) *minor*, M'Coy.

VII. Genus MYRIANITES, M'Leay.

16. *Myrianites tenuis*, M'Coy.

17. — *Murchisoni*, Emmons.

C. APPENDIX.

VIII. Genus CARIDOLITES, Nicholson.

18. *Caridolites Wilsoni*, Nicholson.

May 15, 1873.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

Major Thomas George Montgomerie, B.E., was admitted into the Society.

The following communications were read :—

- I. "Determination of the Number of Electrostatic Units in the Electromagnetic Unit made in the Physical Laboratory of Glasgow University." By DUGALD M'KICHAN, M.A. Communicated by Sir WILLIAM THOMSON, F.R.S. &c. Received April 15, 1873.

(Abstract.)

The object of this paper is to describe experiments made at intervals from 1870 to 1872 in the Physical Laboratory of Glasgow University to determine the relation between the fundamental units in the two systems of absolute electrical measurement, the electromagnetic and the electrostatic. A summary is also given of the results of similar observations made by W. F. King in 1867 and 1868.

The two systems of electrical measurement, or the units which they employ, are founded on the fundamental units of time, mass, and space applied to the observed effects of electricity at rest and electricity in motion. The dimensions of quantity in the two systems are such that the ratio of the electromagnetic and the electrostatic unit of quantity is expressible as a velocity.

This velocity, usually known as v , is not only of great importance in all combinations of electromagnetic and electrostatic action, but it is also of great scientific importance in the theory of the propagation of electromagnetic disturbances through a dielectric medium. It occupies a very important place in the development of the electromagnetic theory of light by Professor Clerk Maxwell, according to whose theory this velocity v is the same as the velocity of light.

The first experimental determination of v was made by Weber from a common electrostatic and electromagnetic measure of capacity. As the result of Weber's experiments, v was found to be 310.74×10^9 centims. per second.

Another determination was made by Prof. Clerk Maxwell in 1868, by means of a direct comparison of electrostatic attraction with electromagnetic repulsion. His experiments gave $v = 288 \times 10^9$ centims. per second.

The value of v given by the experiments here described is 293×10^9 centims. per second. The method employed was that of obtaining an absolute electrostatic and an absolute electromagnetic measurement of the same electromotive force. v is defined as the ratio of the units of quantity in the two systems; but it follows from the definition of electromotive force, that v is also the ratio of the units of electromotive force in the two systems.

The electromotive force, or the difference of potentials between the two poles of a constant Daniell's battery, was measured electrostatically by means of Sir William Thomson's absolute electrometer. The absolute electromagnetic value of this electromotive force was given by the effect of the current which it maintained in the circuit of an electro-dynamometer. The determination of this value depended on the resistance of the electro-dynamometer-circuit, which was reckoned in terms of the absolute value of the British-Association standard unit of resistance. Any correction which may hereafter be found to be applicable to the absolute value of this standard coil, as measured at King's College by Professors Clerk Maxwell, Balfour Stewart, and Fleeming Jenkin, must be applied to the value of v given above.

The comparisons made in 1867 and 1868 by Mr. King gave as the mean value of v , 284.6×10^9 centims. per second.

The experiments made in 1870 with the new absolute electrometer gave as the mean result $v = 294.5 \times 10^9$ centims. per second. The result

of later observations made under much more favourable circumstances was $v = 292.4 \times 10^8$ centims. per second.

The latest observations (1872) furnish the most probable value of v , 293×10^8 centims. per second.

II. "*On Jeypoorite, a Sulph-antimonial Arsenide of Cobalt.*" By Major W. A. Ross, late Royal Artillery. Communicated by Prof. W. H. MILLER, Foreign Sec. R.S. Received April 16, 1873.

A mystery has pervaded the history and character of this almost unknown mineral (the very name of which is mis-spelled "*Syepoorite*"), which was scarcely to have been expected from English mineralogists, as the only place in the world in which it has yet been found is the copper-mines of *Khetree*, in the Hindu principality of Jeypoor, in Rajpootana, India, the Rajah of which territory politely sent the writer the specimens of which an analysis is here submitted.

Notwithstanding Jeypoor is one of the feudatories of the British Government of India, it is believed that there is not a specimen of this mineral in the British Museum; at any rate there was not one in 1868, when Professor Maskelyne even expressed to the writer his doubt of the existence of such a mineral.

Professor Nicol ('Manual of Mineralogy,' p. 458) tells us (1849) that this mineral, which he calls "*Syepoorite*," from "*Syepoor near Rajpootana*"¹ was analyzed by Middleton, who found in it 64.64 per cent. cobalt and 35.36 sulphur*. Dana and other mineralogists have followed this account; and Professor Miller informs me that Middleton's analysis is given in the 'Memoirs of the Chemical Society,' vol. iii. 1845-48, p. 39. There may, of course, be a mineral from the same place answering the description of Middleton's analysis. All I assert is that none of the many crystals examined by me gave the indications of a pure sulphide of cobalt. At first I certainly thought *Jeypoorite* was only an antimonial sulphide of cobalt, not having any reason to doubt further the description given by Nicol; but by a closer and novel method of examination, to be presently described, I succeeded in detecting *arsenic* in considerable quantity.

Plattner ('*Pirbirkunst mit dem Löthrohre*,' vierte Auflage, Leipsic, 1865) attributes to *Jeypoorite*, evidently from the description of another work, a percentage of 65.2 cobalt, with the remainder sulphur, and says of it, at p. 311, "Das Löthrohrverhalten ist nicht bekannt."

* If Middleton had found iron, copper, &c., one might have supposed that he took some of the *general* sand for his analysis, which would of course reduce the percentage of cobalt and raise that of sulphur.

This is about all we know of this mineral in Europe. The natives of the neighbourhood of Khetree call it, if I remember rightly, "Sheta," or "Sheeta," and state that it is used by their jewellers, not only for the usual blue enamels made with cobalt oxide, but to impart to gold a rose-colour—an art, perhaps, worth knowing.

The state and quantity in which it was sent to me almost preclude the possibility of a regular chemical analysis, so that in the mean time I beg to submit the following:—

Pyrological Analysis of Jeypoorite.

(1) *Appearance*.—A dark grey sand with shining metallic-looking points, yellow and white, interspersed. Through the lens, these are found to be semimetallic fragments, the white crystalline, the yellow amorphous; there are also numerous quartzose fragments, white and pink.

(2) About one tenth of this sand is magnetic. Among the portion adhering to the magnet are no metallic crystals, but a considerable quantity of yellowish *pyrrhotine*.

(3) In a glass of phosphoric acid, this magnetized sand gives chiefly reactions of iron.

(4) After the magnetized portion was removed, the remainder (among which were all the metallic-looking crystals) was put into a sieve with holes of the diameter of a small pin, and the larger metallic crystals and fragments, with the greater part of the pieces of quartz, thus separated.

(5) A fragment of the more yellow metallic portion, treated in phosphoric acid, showed that it was *copper pyrites*.

(6) The metallic crystals could now be plainly observed through a lens to be of the cubic system—indeed, for the most part, apparently cubes with bevelled or truncated edges, and some octahedrons. As these modifications give the crystals, compared with the other fragments, a roundish figure, the following expedient was resorted to for the further separation of the former from the latter.

(7) The now coarse sand was placed in the middle of a long stiff paper tray inclined at an angle of about 30° , and the whole of it blown upwards with the jet of a mouth-pyrogene, when the lighter non-metallic fragments were blown away from the rest, the whole moved from its place, and the greater part of the roundish crystalline ones thus made to roll down to the bottom of the tray; as many of the rest as possible were picked out with forceps, and added to those which had rolled down.

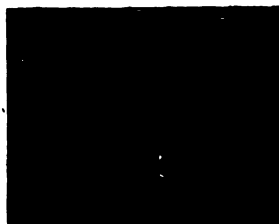
(8) These crystals are the pure *Jeypoorite*, and, when thus collected separately, look (without a lens) like a number of glittering steel beads, each about half the size of a small pin's head. Through a lens they are seen to be of a silver-white, not a steel-grey colour. Their crystalline form is not perceptible without a lens, although the metallic glancing of the smooth upturned side distinguishes them among the other metallic frag-

ments. These crystals weigh from 1.0 to .7 milligramme each*; but 50 of them crushed in a Plattner's steel mortar, produced a powder weighing only 36 mgrs. This powder is blue-black and semimetallic, like that made from galena; it adheres to every thing it touches, soiling paper like graphite—a property not possessed by the crystals. Neither the roasted powder nor the crystal is magnetic.

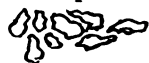
(9) Professor Miller has favoured me with the following determination of the crystalline form:—

“These crystals belong to the cubic system. The simple forms are:—the cube 1 0 0; the octahedron 1 1 1; and a hemihedral form with parallel faces Π 0 1 2. The angles between normals to the faces are:—

0 1 0,	0 0 1	90° 0'
0 0 1,	1 0 0	90 0
1 0 0,	0 1 0	90 0
1 0 0,	1 1 1	54 44
1 0 0,	1 2 0	63 26
0 1 0,	1 2 0	26 34
1 1 1,	1 2 0	39 14.”



(10) Roasted on an aluminium plate (*vide* my paper on “Pyrology,” Proceedings of the Royal Society, No. 137, vol. xx. par. 89) a very slight smell of SO_2 is at first perceived, after which the powder very slowly changes its semimetallic appearance for the sooty black one of protoxide of cobalt; for sulphides, arsenides, &c. thus roasted do not “sinter.” No green indications of *nickel* are given (“Pyrology,” par. 90).

(11) Roasted *through* a platinum-foil tray (*vide* “Pyrology,” par. 83), three crystals of *Jeypoorite* ground into a paste with double the weight of pure Fe_2O_3 (a Freiberg reagent) caused the steel leg of the forceps to be covered thinly with a sublimate disposed in leopard-like spots, white (*antimony*), with an orange tinge round their edges (*arsenic*). 

(a) To make certain of this reaction, a small trace of Sb_2O_3 was mixed intimately with a quantity of the Freiberg rust and water; and when carefully ignited on a new platinum tray, it deposited exactly similar spots on the forceps, only *white* on the edges as elsewhere.

(12) Fused with soda on a platinum wire, and the mass placed on silver foil with water, three crushed crystals of *Jeypoorite* gave a moderately brown stain of sulphide of silver, showing that it contains a small quantity of sulphur*.

* There are, however, much smaller ones in the residue from (4).

† In testing my carbonate of soda previously to using it with *Jeypoorite* for the sulphur and sulphuric-acid test of Berzelius on silver foil, I fused the soda on a “mortar” of beautiful-looking close-grained charcoal from the Government powder-works at Waltham Abbey. The fused soda-mass, dug out as Berzelius recommends, and placed

(13) Three large selected crystals of *Jeypoorite* were added, one by one, to a phosphoric acid bead of 50 mgrs. in an O. P. These weighed 3.2 mgrs. They all at first emitted a few bubbles of gas (SO_2 ?), and afforded yellow creamy streaks (S?) not unlike those of phosphate of silver, but proved not to be due to that metal, by the test described in par. 42 of "Pyrology," and a sooty-black oxide (Co O ?), while the platinum wire was slightly corroded. The first two products were soon dissipated; the cobalt oxide dissolving, gave the glass a fine rose-tint in the case of the first crystal. The second crystal coloured the glass a much deeper red; and when the third had been dissolved in the glass, it assumed the splendid red-violet hue of cobalt (*vide* "Pyrology," par. 35).

(14) It took exactly 2.5 mgrs. of pure cobalt oxide to give to a 50 mgr. $\ddot{\text{P}}$ glass laid alongside of (13) on a sheet of white paper, and also viewed with it by transmitted light, the same colour.

Therefore

$5 : 6.4 :: x \text{ (per cent.)}; \text{ and } x = \frac{5}{6.4} \text{ of } 100, = 83.2 \text{ per cent. cobalt.}$

(15) It took 21 mgrs. of carbonate of soda to *azurize* bead (14), but only 20 mgrs. to *azurize* (13); so that there was slightly more acid in the former bead.

(16) The bead (14) weighed, after the soda had been added, 77 mgrs., while (13) bead weighed 75 mgrs. They had both been kept up to the mark by the means mentioned in par. 78 of the paper on "Pyrology."

(17) As this percentage of cobalt seemed too high compared with that recorded in works on mineralogy from Middleton's analysis above cited, I made the following second assay, in a manner slightly differing from the preceding. In order also to guard against the possibility of loss of any cobalt from its fusion in the form of arsenide with the red-hot platinum-wire, the glass with a *Jeypoorite* crystal on its surface was manipulated in such a manner that the lower surface of the bead just touched the upper surface of the jet, in which position the ring of the platinum-wire never even approaches a red heat, being kept comparatively cool by the action of the superposed blast (par. 7, "Pyrology"), which cuts the heat off from it;

while sufficient heat is imparted to the $\ddot{\text{P}}$ glass to dissolve most substances, which, even metals, cannot under these conditions attack or combine with the platinum, although, lightened by the loss of volatile

on bright silver foil with distilled water, gave a brown stain of AgS quite as strong as that afterwards afforded by *Jeypoorite*, to which, therefore, if this soda had been used, double the amount of sulphur it really contains might have been attributed. The same soda fused on platinum wire gave no stain whatever to silver.

A glass of pure boric acid fused in O.P. on this charcoal detects in its ash ("Pyrology," par. 65) *silica*, *lime* (in considerable quantity), *magnesia*, and *protoxide of iron*; while soda, as above-mentioned, shows it to contain *sulphur*, or sulphuric acid, and also shows its iron, which gives the soda a pale salmon-colour. The Government gunpowder is, I believe, made of this charcoal.

matter, or raised by the boiling of fresh \ddot{P} added, the metallic mass or ball rises to or touches the platinum wire. The glass is thus made to act as a miniature test-tube, with this advantage, that any novel reaction or peculiarity can, by removing it from the pyrocone, be immediately preserved for inspection, like a fly in amber.

(18) (a) 2.4 mgrs. of pure cobalt oxide were taken to standardize an assumed 50-mgr. \ddot{P} glass with an apparent red-violet colour.

(b) It took four *Jeypoorite* crystals, weighing 3.2 mgrs., to bring another \ddot{P} glass, also measured only by the eye, to a similar colour*.

(c) [a] glass weighed 76.5 mgrs., and [b] glass 79.2 mgrs.

(d) Then [a]=3.33 (standard of Co), and [b]=4 per cent.

(e) Therefore $3.3 : 4 :: x$ (per cent.); and $x = \frac{3.3}{4}$ of 100, =82.5 per cent. of cobalt.

(19) I have not the means as yet of separating and estimating by my methods small quantities of sulphur and sublimates; but, assuming the percentage of cobalt oxide in *Jeypoorite* to be about 82, of the remaining 18 I am led to believe there are nearly equal quantities of arsenic and antimony, with very little sulphur—certainly not more than 5 per cent.; so that I would temporarily, until a sufficient quantity of the *pure* mineral (not the sand) be obtained to submit it to a regular chemical analysis, estimate the constituents of *Jeypoorite* to be about

	per cent.
Oxide of Cobalt	82
„ Antimony	7
„ Arsenic	6
Sulphur	5
	<hr/> 100

(20) The garlic smell of volatilizing arsenic is only obtainable from *Jeypoorite* by heating the mineral carefully with a ball of gold upon charcoal. Plattner used gold for separating copper from its arsenide and that of nickel (*vide* his 'Nickel and Cobalt Assay,' page 616 *et seq.*); but he *fuses* the arsenides with the gold in borax, in which condition it would appear that the gold takes up some arsenic and retains it (*vide* Gmelin's 'Chemistry,' vol. vi. p. 238, English translation). My use of gold, on the contrary, is more mechanical than chemical, for the gold ball (weighing about 100 mgrs.) is placed in a round cavity made in a charcoal mortar ("Pyrology," par. 88) slightly larger than its diameter, and a crystal of *Jeypoorite* put on the offside or rear, *i. e.* the side away from the blast. The whole front of the mortar is now covered with an H. P. not too strong (fig. 8, "Pyrology"), when the gold becomes red-hot, but does *not fuse*. The cobalt of the arsenide has now a *tendency* to reduction, and to join the gold in a metallic state, while the arsenic is volatilized by

* Neither of these glasses was coloured up to the standard of 5 per cent.

this amount of heat; and when the glowing charcoal, in the case of *Jeypoorite*, is afterwards held under the nose, a distinct and even strong smell of garlic is perceived. If fusion of the gold takes place, this smell ceases immediately. There is also emitted from *Jeypoorite* thus treated another very peculiar but indescribable smell, after that of garlic has ceased. This I believe to be due to volatilizing *antimony*; and the fact is, so far as I know, unnoticed by writers on the "blowpipe" since the time of Cronstedt; but Von Engestrom says of this metal (Translation of Cronstedt's 'Mineralogy,' by Gustav von Engestrom, London, 1772, p. 308), "Antimony is known by its particular smell, easier to be distinguished, when once known, than described." The corrosion of platinum by *Jeypoorite* powder, mixed with a large amount of rust, when roasted *through* it, is also (in my opinion) a strong proof of the existence of sulphide of antimony (*vide* "Pyrology," par. 80); for in this case I found that, thus roasted, *Jeypoorite* considerably, and even pure Sb_2O_3 , slightly corroded the platinum, while native arsenic and arsenic acid did not do so at all.

(21) When the gold ball, placed with a crystal of *Jeypoorite* on charcoal, as described in (20), is *fused* by an O.P., the surrounding ash of the charcoal is observed to be tinged with a beautiful rose tint. This phenomenon appears to be due to the formation of arseniate of cobalt, and to be analogous to that of *Erythrine* in nature, where the cobalt oxide, combining with arsenic acid, is hydrated with the water absorbed chemically from the atmosphere by the latter. Cronstedt ('Essay towards a System of Mineralogy,' Lond. 1772, Sect. ccxvi. p. 230) notices this fact under the article "Cobalt," which, he says, "when united with the calx of arsenic in a slow (not a brisk) calcining heat, assumes a red colour: the same colour is naturally produced by way of efflorescence, and is then called the *bloom* or *flowers of cobalt*." The fact proves the truth of Gmelin's assertion, that fusing gold takes up a certain portion of arsenic. Another proof seems shown in the fact that the gold ball thus treated, assumes on the surface, when rapidly and momentarily heated, a dark reddish, and even a copper-coloured yellow, which may be the so-called "rose-colour" said to be given to gold by Indian goldsmiths, but which I have never yet seen in any part of India.

III. "On a Periodicity of Rainfall in connexion with the Sun-spot Periodicity." By C. MELDRUM, Director of the Meteorological Observatory, Mauritius. Communicated by Sir EDWARD SABINE, K.C.B., F.R.S. Received April 6, 1873.

Assuming that there is a sun-spot periodicity, in the course of which the sun undergoes a variation with respect to heat or some other form of energy, we should expect to find a corresponding variation in the state of our atmosphere.

With this idea, it was some time ago determined to discuss the cyclones that had occurred during the last twenty-five years in the Southern Indian Ocean, and it was found, what had been often surmised, that they were more frequent and more violent in the maxima than in the minima sun-spot years.

It is well known that the cyclones of the Indian Ocean are attended with much rain, which is not confined to the body of the storm, but extends over wide areas. Years remarkable for cyclones, therefore, should be also years remarkable for rain; but to test this inference, with regard to the Indian Ocean, we had no rainfall statistics, except eighteen years' observations at Mauritius: and these were in every respect favourable, the rainiest years having been those in which cyclones were most abundant. In the absence of other data, the Brisbane and Adelaide rainfalls were consulted; and it was found that, like the Mauritius rainfall, they indicated a periodicity. It was then surmised that there might be a rainfall periodicity generally, and that, if such was the case, both it and the cyclone periodicity were concomitant effects of one and the same cause. This supposition having been strengthened by the results of an examination of the rainfall of England, it was resolved to examine all the rainfall-tables (containing one or more sun-spot periods) that could be obtained.

The first step was to ascertain whether there was any marked difference in the amount of rainfall at or about the turning-points of the sun-spot period. This has been done for eighteen stations by adding the rainfall in the three principal years of maxima, and in the three principal years of minima sun-spots, and then comparing the results. The following Table gives the stations and the years of observation:—

TABLE I.

Stations.	Lat.	Long.	Years of Observation.	
			From	To
1. Christiania (Observatory)	59 55 N.	10 43 E.	1839	1863
2. Aberdeen	57 09 N.	2 06 W.	1829	1867
3. Arbroath	56 34 N.	2 35 W.	1843	1868
4. Deanston House, Perthshire	1837	1865
5. Inveresk	55 56 N.	3 03 W.	1852	1865
6. Carbeth-Guthrie, near Glasgow	1815	1859
7. Haddington	1836	1864
8. Greenwich (Observatory).....	51 29 N.	0 0	1815	1869
9. Brussels (Observatory).....	50 51 N.	4 22 E.	1833	1861
10. Toronto (Observatory).....	43 29 N.	75 04 W.	1854	1869
11. Calcutta	22 33 N.	88 21 E.	1853	1871
12. Madras (Observatory)	13 04 N.	80 14 E.	1843	1849
13. Demerara (Observatory)	6 49 N.	58 15 W.	1846	1856
14. Mauritius (Observatory)	20 10 S.	57 30 E.	1854	1872
15. Brisbane (Observatory)	27 28 S.	153 06 E.	1860	1871
16. Natal (Observatory).....	29 30 S.	38 02 E.	1858	1867
17. Cape (Observatory)	33 56 S.	18 49 E.	1842	1869
18. South Australia (Adelaide).....	34 54 S.	138 38 E.	1839	1860

The annual rainfall at these stations is given in a subjoined Table, from which we obtain the following results, showing the rainfall at each station for maximum- and minimum-sunspot periods of three years each, the maximum- and minimum-sunspot years being respectively placed in the middle of each three-year period, as will be seen on reference to the Table.

(1) CHRISTIANIA.

Max. years.	Min. years.
in.	in.
64·65	59·75
72·88	57·19
<hr/> 137·53	<hr/> 116·94

(2) ABERDEEN.

88·42	49·26
77·38	71·54
79·30	94·14
96·27	90·35
<hr/> 341·37	<hr/> 305·29

(3) ARBROATH.

79·4	79·7
85·0	78·2
<hr/> 164·4	<hr/> 157·9

(4) DEANSTON HOUSE.

111·25	104·22
121·85	100·95
<hr/> 233·10	<hr/> 205·17

(5) HADDINGTON.

86·1	70·3
72·5	77·7
<hr/> 158·6	<hr/> 148·0

(6) INVERESK.

85·82	76·79
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(7) CARBETH-GUTHRIE.

125·94	130·84
132·57	131·09
139·79	124·60
133·92	103·48
<hr/> 532·22	<hr/> 490·01

(8) GREENWICH.

Max. years.	Min. years.
in.	in.
77·45	82·31
88·26	82·84
71·76	63·08
73·76	67·10
78·30	84·20
<hr/> 389·53	<hr/> 379·53

(9) BRUSSELS.

85·26	74·43
91·23	95·04
92·12	75·54
<hr/> 268·61	<hr/> 245·01

(10) TORONTO.

102·22	110·18
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(11) CALCUTTA.

210·37	218·76
--------	--------

(12) MADRAS.

175·0	125·0
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(13) DEMERARA.

331·46	275·94
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(14) MAURITIUS.

170·77	132·33
134·44	120·72
<hr/> 305·21	<hr/> 253·05

(15) QUEENSLAND.

124·07	98·25
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(16) NATAL.

81·41	91·85
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(17) CAPE.

81·3	64·5
91·2	69·2
<hr/> 172·5	<hr/> 133·7

(18) SOUTH AUSTRALIA.

64·44	52·90
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We take for the maximum-sunspot years 1817, 1830, 1837, 1840, and 1860; and for the minima-years 1823, 1834, 1844, 1856, and 1867.

In some instances we cannot get three-year periods with the maximum

or minimum year in the middle. This happens in the following cases:— In the Aberdeen series, 1865–67 has been taken for 1866–68, because we have not the observations for 1868. In the Deanston-House series 1837–39 has been taken for 1836–38, the series commencing with 1837. In the Demerara observations 1854–56 has been taken in place of 1855–57, the series ending with 1856. The years 1870–72 have, in the Mauritius series, been taken for three maxima-years; but if 1872 be the maximum-sunspot year, and the three principal maxima-years be 1871–73, the rainfall for these years will probably be still greater; for since the 1st of January 1873, 38 inches of rain have fallen. The means of two years have been taken in the Brisbane observations, the series commencing with 1860 and ending with 1871. In the Calcutta and Toronto series we have taken the one maximum-period, and the mean of the two minima-periods.

These slight deviations, as will be afterwards seen, do not affect the general results.

Now, collecting the sums of the rainfall in the maxima- and minima-periods of three years each, we get the following Table:—

TABLE II.

Stations.	Rainfall in max. periods.	Rainfall in min. periods.	Excess or defect in max. periods on min. periods.	Rainfall in max. years.	Rainfall in min. years.	Excess or defect in max. years on min. years.
	in.	in.	in.	in.	in.	in.
Christiania	137'53	116'94	+ 20'59	43'87	39'90	+ 3'97
Aberdeen	341'37	305'29	+ 36'08	110'64	108'92	+ 1'72
Arbroath	164'40	157'90	+ 6'50	53'80	58'50	— 4'70
Deanston	233'10	205'17	+ 27'93	76'90	66'13	+ 10'77
Inveresk	85'82	76'79	+ 9'03	31'97	29'75	+ 2'22
Carbeth	532'22	490'01	+ 42'21	175'71	168'30	+ 7'41
Haddington	158'60	148'00	+ 10'60	24'60	21'30	+ 3'30
Greenwich	389'57	379'53	+ 10'04	132'74	120'39	+ 12'35
Brussels	268'61	245'01	+ 23'60	87'82	83'01	+ 4'81
Toronto	102'20	106'90	— 4'70	27'99	28'05	— 0'06
Calcutta	210'37	218'76	— 8'39	52'61	64'23	— 11'62
Madras	175'00	125'00	+ 50'00	54'00	45'00	+ 9'00
Demerara	331'46	275'94	+ 55'52	132'22	87'73	+ 44'49
Mauritius	305'21	254'06	+ 51'15	92'43	82'20	+ 10'23
Queensland	124'07	98'28	+ 25'79	54'63	61'04	— 6'41
Natal	81'41	91'85	— 10'44	30'60	31'49	— 0'89
Cape	172'50	133'70	+ 38'80	53'70	40'70	+ 13'00
Adelaide	64'44	52'90	+ 11'54	25'44	19'67	+ 5'77
Sums	3877'88	3482'03	+ 395'85	1261'67	1136'31	+ 105'36

The above Table comprises the results of all the rainfall-tables I can find containing a maximum- and a minimum-period. Now we see that, in the maximum-sunspot periods of three years each, the rainfall at the eighteen stations was 395'85 inches greater than in the minimum-sunspot

periods of three years each, and, which is still more striking, that the excess is on the side of the maxima-periods at fifteen out of the eighteen stations.

In the last three columns I have given the total rainfall for the single maxima-years 1817, 1830, 1837, 1849, and 1860, and for the single minima-years 1823, 1834, 1844, 1856, and 1857, with the excesses and defects; and we find a total excess of 105·36 inches in favour of the maxima-years, only five stations showing an excess on the other side.

The unfavourable stations are Toronto, Calcutta, and Natal; and it should be remarked that at Toronto the snow was not melted, but one tenth of its depth taken to represent an equivalent depth of water, while Mr. Blarifone, the meteorological reporter to the Bengal Government, has expressed a doubt as to the accuracy of the Calcutta observations in the earlier part of the series. If, however, we had taken the years 1847-49 in place of 1848-50 for the maximum-period, the Calcutta observations would have given a different result. With regard to Natal, the geographical peculiarities described by Dr. Mann probably render that station unfavourable for showing in a few years any periodicity that may exist.

Having ascertained, then, that there is at nearly all the above stations a very remarkable difference in the amount of rainfall at or near the turning-points of the sun-spot period, the next step is to find the effect of grouping the observations. For this purpose we give, in the first place, the rainfall at as many stations as possible in each minimum-sun-spot year, and in each two or three years on either side of it; and, in the second place, we give the rainfall for each maximum-year and two or three years on either side of it. If we do not find any tendency to an equalization of the annual rainfall, we shall have further proof of a periodicity.

TABLE III. (Minimum-year, 1834).

Stations.	1831.	1832.	1833.	1834.	1835.	1836.
	in.	in.	in.	in.	in.	in.
Aberdeen	29'16	21'07	22'04	12'28	14'94	24'69
Carbeth.....	49'59	40'29	46'12	42'37	42'60	53'90
Greenwich	34'48	30'69	29'79	23'84	29'21	31'01
Total	113'23	92'05	97'95	78'49	86'75	109'60
Means	34'74	30'68	32'65	26'16	28'92	36'53

The above Table shows no tendency to an equalization, but to the formation of a curve, the lowest point of which corresponds with the minimum-sunspot year.

TABLE IV. (Minimum-year, 1844).

Stations.	1841.	1842.	1843.	1844.	1845.	1846.
	in.	in.	in.	in.	in.	in.
Christiania	24'85	12'93	19'15	19'63	23'57	21'14
Aberdeen	27'38	25'39	22'84	21'70	27'00	30'30
Deanston	32'15	30'80	30'70	29'38	44'14	44'10
Carbeth.....	47'42	37'39	36'89	37'69	50'02	49'38
Haddington	24'30	17'30	22'30	21'30	26'70	29'80
Greenwich	28'76	20'03	22'52	21'87	18'69	23'54
Brussels	30'72	24'77	31'63	31'55	31'86	24'96
South Australia	17'96	20'32	17'19	16'88	18'83	26'88
Sums.....	233'54	188'93	200'62	200'00	240'81	250'10
Means	29'19	23'62	25'08	25'00	30'10	31'26

The above Table gives results similar to those of Table II.; only the year of minimum rainfall is 1842 in place of 1844.

TABLE V. (Minimum-year, 1856).

Stations.	1853.	1854.	1855.	1856.	1857.	1858.	1859.
	in.	in.	in.	in.	in.	in.	in.
Christiania ...	12'89	17'69	19'15	20'27	17'77	20'42	20'78
Aberdeen	21'33	16'45	23'29	43'78	27'07	28'29	30'65
Arbroath	27'60	20'90	21'20	32'20	24'80	24'40	25'20
Deanston	38'70	42'15	28'65	36'75	35'55	40'40	43'85
Inveresk	28'33	21'68	21'43	29'76	25'61	23'58	25'34
Haddington ...	23'40	23'10	21'00	28'50	28'20	23'20	24'70
Greenwich ...	29'00	19'00	23'80	21'90	21'40	17'80	25'90
Brussels	26'65	28'67	26'15	31'34	18'05	19'92	29'68
Mauritius.....	39'82	39'43	42'66	46'23	43'44	35'50	56'87
Cape	21'20	20'00	24'60	21'90	22'70	24'10	36'70
South Australia	26'99	15'34	23'14	24'92	21'16	21'52	14'84
Sums.....	295'31	264'31	275'07	337'55	285'75	279'13	334'51
Means	26'85	24'03	25'01	30'69	25'89	25'38	30'41

The above Table makes 1854 the driest year, and 1856 and 1859 the wettest years, although 1856 was the minimum-sunspot year. From 1854 to 1856 the rainfall increased, and then decreased till 1859. All the eleven stations except three are in Europe. I have compiled from vol. i. of the 'Report of the U.S. Commissioner of Patents' the following Table, showing the rainfall at eleven stations in the United States in the years 1854-59:—

TABLE VI.

Stations.	1854.	1855.	1856.	1857.	1858.	1859.
	in.	in.	in.	in.	in.	in.
Amherst	47'34	48'18	40'13	47'66	42'57	51'28
Nantucket... ..	47'43	48'94	35'70	49'87	41'94	48'28
New Bedford	47'84	36'44	32'98	38'59	42'42	49'91
Glenwood	43'03	44'10	39'50	43'45	53'68	45'59
Gettysburg	33'22	47'62	28'62	38'22	45'64	42'42
Moinsville	45'00	43'10	30'34	47'80	40'10	51'65
Pocopsou	45'90	49'56	34'35	46'72	41'35	50'66
Lambertville	43'13	45'17	32'32	48'66	40'31	47'63
New Harmony	35'57	48'07	23'18	39'66	48'71	44'33
Pean Yau	19'66	29'96	21'68	44'90	26'78	29'15
Providence	37'10	39'05	40'97	44'75	44'51	45'16
Sums	445'22	480'19	359'77	490'28	468'01	506'06
Means	40'47	43'60	32'71	44'57	42'55	46'01

These American observations are the only additional ones we have for the above period; and they make 1856 the minimum-rainfall year. Combining the means in Tables V. and VI., we get:—

TABLE VII.

Years	1854.	1855.	1856.	1857.	1858.	1859.
	in.	in.	in.	in.	in.	in.
Means	32'25	34'33	31'70	34'73	33'96	38'21

showing that the means of twenty-two stations still make 1856 the minimum-year.

TABLE VIII. (Minimum-year, 1867).

Stations.	1863.	1864.	1865.	1866.	1867.	1868.
	in.	in.	in.	in.	in.	in.
Arbroath	24'70	23'90	27'90	25'70	30'50	33'60
Greenwich	19'80	16'90	28'70	30'70	28'40	25'10
Mauritius	33'42	24'15	44'73	20'57	35'97	64'18
Brisbane	68'82	47'00	24'11	37'24	61'04	35'98
Cape	25'60	18'90	18'70	19'20	22'90	22'90
Sums	172'34	140'85	144'14	133'41	178'81	181'76
Means	34'47	28'17	28'83	26'68	35'76	36'35

The driest year at these stations was 1866; and 1867 was one of the rainiest years.

Collecting the means in Tables III., IV., V., and VI., we get the next Table, showing the mean rainfall in the years 1834, 1844, 1856, and 1866, and in the years on either side of them.

TABLE IX.

Years.	Rainfall.	Means.
	in.	in.
1832, 1842, 1854, 1864.....	114·72	28·68
1833, 1843, 1855, 1865.....	120·89	30·22
1834, 1844, 1856, 1866.....	109·53	27·38
1835, 1845, 1857, 1867.....	129·51	32·38
1836, 1846, 1858, 1868.....	137·69	34·42

Taking now each maximum-sunspot year, and two or three years on either side of it, we get:—

TABLE X. (Maximum-year, 1837).

Stations.	1834.	1835.	1836.	1837.	1838.
	in.	in.	in.	in.	in.
Aberdeen	12·28	14·94	24·69	20·29	32·40
Carbeth	42·37	42·61	53·90	43·16	42·73
Greenwich	23·84	29·21	31·01	19·11	21·64
Brussels	20·12	24·33	32·59	29·15	23·52
Sums	98·61	111·08	142·19	111·71	120·29
Means	24·65	27·77	35·55	27·93	30·07

TABLE XI. (Maximum-year, 1848.6).

Stations.	1846.	1847.	1848.	1849.	1850.	1851.
	in.	in.	in.	in.	in.	in.
Christiania	21·14	14·65	25·00	17·16	20·49	18·48
Aberdeen	30·30	19·50	32·70	25·10	21·50	21·16
Arbroath	29·70	25·30	33·00	23·50	22·90	23·30
Deanston	44·10	37·65	42·60	40·00	39·25	32·47
Haddington	29·80	20·70	28·90	22·40	21·20	22·40
Carbeth	49·38	39·57	49·81	41·71	42·40	37·45
Greenwich	23·54	12·82	30·20	23·90	19·70	21·60
Brussels	24·95	24·07	31·32	26·97	32·94	30·39
Demerara	85·65	89·98	101·90	132·22	97·34	102·84
Cape	22·50	22·40	23·20	24·60	31·50	20·30
South Australia	26·88	27·61	19·73	25·44	19·27	30·63
Sums	387·94	334·25	418·36	403·00	372·49	361·02
Means	35·57	30·39	38·03	36·64	33·86	32·82

TABLE XII. (Maximum-year, 1860).

Stations.	1858.	1859.	1860.	1861.	1862.	1863.
	in.	in.	in.	in.	in.	in.
Christiania	20'42	20'78	26'71	25'39	25'12	21'79
Aberdeen	28'29	30'65	34'65	30'97	30'77	25'74
Arbroath	24'40	25'20	30'30	29'50	31'20	24'70
Deanston	40'40	43'85	37'30	45'05	51'55	44'55
Haddington	23'20	24'70	27'30	25'90	29'80	23'00
Inveresk	23'58	25'34	31'97	28'51	32'89	29'10
Greenwich	17'80	25'90	32'00	20'40	26'50	19'80
Mauritius	35'51	56'87	45'16	68'73	28'40	33'42
Cape	24'10	36'70	29'10	25'40	32'01	25'60
Sums	237'70	290'04	294'49	299'85	288'24	247'90
Means	26'41	32'23	32'72	33'32	32'03	27'54

Combining the means in Tables X., XI., and XII., we get :—

TABLE XIII.

Years.	Rainfall.	Means.
	in.	in.
1834, 1846, 1859	92'15	30'72
1835, 1847, 1860	90'88	30'29
1836, 1848, 1861	107'50	35'83
1837, 1849, 1862	96'60	32'20
1838, 1850, 1863	92'36	30'79

The last four Tables show an increase of rain at or very near the maximum-sunspot year; and there is, upon the whole, a distinct tendency to an ascent and descent.

Comparing Tables IX. and XIII., we find that the mean rainfall in the minimum-sunspot years 1834, 1844, 1856, and 1866 was 27'38 inches, and in the maximum-sunspot years 1836, 1848, and 1861, 35'83 inches, giving an excess of 8'45 inches.

With the exception of 1842, all the years of minimum and maximum rainfall shown by the Tables are within a fraction of the corresponding minimum- and maximum-sunspot year.

The last Table I beg to present is one showing the annual fall at eighteen stations. An inspection of it will show that, generally, the maxima-periods of those years give each more rainfall than any other three consecutive years, and the minima-periods less rain than any other three consecutive years, in each set of observations.

There are no doubt errors of observation, as well as discrepancies, arising from changing the forms and positions of the gauges; and it is difficult to believe that more correct observations would not have given still more remarkable results.

TABLE XIV.

Giving the Annual Rainfall at Eighteen Stations in different parts of the World.

Years*.	Rainfall.	Years.	Rainfall.	Years.	Rainfall.
Station.—CHRISTIANIA.		in.		in.	
1839.....	21'70	1852.....	32'28	1841.....	32'15
1840.....	21'70	1853.....	21'33	1842.....	30'80
1841.....	24'85	1854.....	16'45	n1843.....	30'70
1842.....	12'93	n1855.....	23'29	n1844.....	29'38
n1843.....	16'55	n1856.....	43'78	n1845.....	44'14
n1844.....	19'63	n1857.....	27'07	1846.....	44'10
n1845.....	23'57	1858.....	28'29	1847.....	37'65
1846.....	21'14	x1859.....	30'65	x1848.....	42'60
1847.....	14'65	x1860.....	34'65	x1849.....	40'00
x1848.....	25'00	x1861.....	30'97	x1850.....	39'25
x1849.....	17'16	1862.....	30'77	1851.....	32'47
x1850.....	22'49	1863.....	25'94	1852.....	54'34
1851.....	18'48	1864.....	27'95	1853.....	38'70
1852.....	20'18	n1865.....	27'68	1854.....	42'15
1853.....	12'90	n1866.....	31'16	n1855.....	28'65
1854.....	17'69	n1867.....	31'51	n1856.....	36'75
n1855.....	19'15	ABERROATH.		n1857.....	35'55
n1856.....	20'27	n1843.....	25'1	1858.....	40'40
n1857.....	17'77	n1844.....	26'3	x1859.....	43'85
1858.....	20'42	n1845.....	28'3	x1860.....	37'30
x1859.....	20'78	1846.....	29'7	x1861.....	45'05
x1860.....	26'71	1847.....	25'3	1862.....	51'55
x1861.....	25'39	x1848.....	33'0	1863.....	44'55
1862.....	25'12	x1849.....	23'5	1864.....	41'90
1863.....	21'79	x1850.....	22'9	1865.....	33'75
ABERDEEN.		1851.....	23'3	HADDINGTON.	
x1829.....	28'66	1852.....	29'3	x1836.....	32'7
x1830.....	30'60	1853.....	27'6	x1837.....	24'6
x1831.....	29'16	1854.....	20'9	x1838.....	28'8
1832.....	21'07	n1855.....	21'2	1839.....	21'4
n1833.....	22'04	n1856.....	32'2	1840.....	23'9
n1834.....	12'28	n1857.....	24'8	1841.....	24'3
n1835.....	14'94	1858.....	24'4	1842.....	17'3
x1836.....	24'69	x1859.....	25'2	n1843.....	22'3
x1837.....	20'29	x1860.....	30'3	n1844.....	21'3
x1838.....	32'40	x1861.....	29'5	n1845.....	26'7
1839.....	31'08	1862.....	31'2	1846.....	29'8
1840.....	24'61	1863.....	24'7	1847.....	20'7
1841.....	27'38	1864.....	33'9	x1848.....	28'9
1842.....	25'39	1865.....	27'9	x1849.....	22'4
n1843.....	22'84	n1866.....	25'7	x1850.....	21'2
n1844.....	21'70	n1867.....	30'5	1851.....	22'4
n1845.....	27'00	n1868.....	33'6	1852.....	27'5
1846.....	30'30	DEAKSTON HOUSE,		1853.....	23'4
1847.....	19'50	Perthshire.		1854.....	23'1
x1848.....	32'70	x1837.....	36'90	n1855.....	21'0
x1849.....	25'10	x1838.....	39'15	n1856.....	28'5
x1850.....	21'50	x1839.....	35'20	n1857.....	28'2
1851.....	21'16	1840.....	25'95	1858.....	23'2
				x1859.....	24'7

* The years taken for maximum are marked x, those for minimum n.

TABLE XIV. (continued).

Years.	Rainfall.	Years.	Rainfall.	Years.	Rainfall.
	in.		in.		in.
x1860.....	27'3	x1845.....	50'02	1852.....	34'20
x1861.....	25'9	1846.....	49'38	1853.....	29'00
1862.....	29'8	1847.....	39'57	1854.....	19'00
1863.....	23'0	x1848.....	49'81	x1855.....	23'80
1864.....	26'1	x1849.....	41'71	x1856.....	21'90
		x1850.....	42'40	x1857.....	21'40
INVERSK.		1851.....	37'45	1858.....	17'80
1852.....	30'17	1852.....	51'55	x1859.....	25'90
1853.....	28'33	1853.....	40'18	x1860.....	32'00
1854.....	21'68	1854.....	42'48	x1861.....	20'40
x1855.....	21'43	x1855.....	30'54	1862.....	26'50
x1856.....	29'75	x1856.....	40'34	1863.....	19'80
x1857.....	25'61	x1857.....	32'60	1864.....	16'90
1858.....	23'58	1858.....	43'29	1865.....	28'70
x1859.....	25'34	1859.....	39'34	x1866.....	30'70
x1860.....	31'97	GREENWICH.		x1867.....	28'40
x1861.....	28'51	1815.....	20'52	x1868.....	25'10
1862.....	32'89	x1816.....	27'49	1869.....	24'00
1863.....	29'10	x1817.....	26'59	BRUSSELS.	
1864.....	29'48	x1818.....	23'37	x1833.....	29'98
1865.....	27'78	1819.....	28'24	x1834.....	20'12
CARBETH-GUTHRIE, near Glasgow.		1820.....	25'70	x1835.....	24'33
1815.....	41'39	1821.....	31'53	x1836.....	32'59
x1816.....	39'59	x1822.....	24'95	x1837.....	29'15
x1817.....	44'96	x1823.....	24'38	x1838.....	23'52
x1818.....	41'39	x1824.....	32'98	1839.....	30'62
1819.....	42'84	1825.....	22'25	1840.....	25'77
1820.....	40'62	1826.....	20'71	1841.....	30'72
1821.....	47'37	1827.....	22'38	1842.....	24'77
x1822.....	45'32	1828.....	28'44	x1843.....	31'63
x1823.....	47'90	x1829.....	22'64	x1844.....	31'55
x1824.....	37'62	x1830.....	31'14	x1845.....	31'86
1825.....	36'48	x1831.....	34'48	1846.....	24'95
1826.....	32'21	1832.....	30'69	1847.....	24'07
1827.....	45'96	x1833.....	29'79	x1848.....	31'32
1828.....	45'30	x1834.....	23'84	x1849.....	26'97
x1829.....	37'10	x1835.....	29'21	x1850.....	32'94
x1830.....	45'88	x1836.....	31'01	1851.....	30'39
x1831.....	49'59	x1837.....	19'11	1852.....	35'00
1832.....	40'29	x1838.....	21'64	1853.....	26'05
x1833.....	46'12	1839.....	27'09	1854.....	28'67
x1834.....	42'37	1840.....	18'43	x1855.....	26'15
x1835.....	42'60	1841.....	28'76	x1856.....	31'34
x1836.....	53'90	1842.....	20'03	x1857.....	18'05
x1837.....	43'16	x1843.....	22'52	1858.....	19'92
x1838.....	42'73	x1844.....	21'87	x1859.....	29'68
1839.....	46'95	x1845.....	18'69	x1860.....	31'70
1840.....	39'89	1846.....	23'54	x1861.....	30'74
1841.....	47'42	1847.....	12'82	TORONTO.	
1842.....	37'39	x1848.....	30'20	1854.....	32'71
x1843.....	36'89	x1849.....	23'90	x1855.....	41'55
x1844.....	37'69	x1850.....	19'70		
		1851.....	21'60		

TABLE XIV. (continued).

Years.	Rainfall.	Years.	Rainfall.	Years.	Rainfall.
	in.		in.		in.
1857.....	40'58	1855.....	93'32	1844.....	18'8
1858.....	32'59	1856.....	87'73	1845.....	20'9
1859.....	39'76			1846.....	22'5
1860.....	27'99	MAURITIUS.		1847.....	22'4
1861.....	34'47	1853.....	39'83	1848.....	23'2
1862.....	34'07	1854.....	39'43	1849.....	24'6
1863.....	32'77	1855.....	42'66	1850.....	33'5
1864.....	36'95	1856.....	46'23	1851.....	20'3
1865.....	32'93	1857.....	43'44	1852.....	23'2
1866.....	39'42	1858.....	35'51	1853.....	21'2
1867.....	30'06	1859.....	56'87	1854.....	20'0
1868.....	34'28	1860.....	45'17	1855.....	24'6
1869.....	39'64	1861.....	68'73	1856.....	21'9
		1862.....	28'40	1857.....	22'7
CALCUTTA.		1863.....	33'42	1858.....	24'1
1853.....	52'08	1864.....	24'15	1859.....	36'7
1854.....	66'47	1865.....	44'73	1860.....	29'1
1855.....	70'37	1866.....	20'57	1861.....	25'4
1856.....	64'23	1867.....	35'97	1862.....	32'0
1857.....	68'97	1868.....	64'18	1863.....	25'6
1858.....	59'76	1869.....	54'57	1864.....	18'9
1859.....	68'66	1870.....	45'57	1865.....	18'7
1860.....	52'61	1871.....	41'61	1866.....	19'2
1861.....	89'10	1872.....	47'26	1867.....	22'9
1862.....	73'48			1868.....	22'9
1863.....	61'15	QUEENSLAND (Brisbane).		1869.....	32'2
1864.....	84'22	1860.....	54'63		
1865.....	61'58	1861.....	69'44	SOUTH AUSTRALIA	
1866.....	65'74	1862.....	28'27	(Adelaide).	
1867.....	76'72	1863.....	68'82	1839.....	19'84
1868.....	91'49	1864.....	47'00	1840.....	24'11
1869.....	66'04	1865.....	24'11	1841.....	17'95
1870.....	60'26	1866.....	37'24	1842.....	30'32
1871.....	93'31	1867.....	61'04	1843.....	17'19
		1868.....	35'98	1844.....	16'88
MADRAS.		1869.....	54'36	1845.....	18'83
1843.....	41'0	1870.....	79'06	1846.....	26'88
1844.....	45'0	1871.....	45'43	1847.....	27'61
1845.....	39'0			1848.....	19'73
1846.....		NATAL (Maritzburg).		1849.....	25'44
1847.....	81'0	1858.....	27'42	1850.....	19'27
1848.....	40'0	1859.....	28'40	1851.....	30'63
1849.....	54'0	1860.....	30'60	1852.....	27'34
		1861.....	22'41	1853.....	26'88
DEMERARA.		1862.....	29'91	1854.....	15'35
1846.....	85'65	1863.....	34'66	1855.....	23'14
1847.....	87'98	1864.....	37'31	1856.....	24'92
1848.....	101'90	1865.....	31'08	1857.....	21'16
1849.....	132'22	1866.....	29'28	1858.....	21'52
1850.....	97'34	1867.....	31'49	1859.....	14'84
1851.....	102'84			1860.....	19'67
1852.....	94'54	CAPE (Observatory).			
1853.....	74'84	1842.....	26'3		
1854.....	94'89	1843.....	24'8		

IV. "On the Heating of a Disk by rapid Rotation *in vacuo*." By BALFOUR STEWART, M.A., F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and P. G. TAIT, M.A., Professor of Natural Philosophy in the University of Edinburgh. Received April 10, 1873.

26. In two previous communications (Proc. Roy. Soc. June 15, 1865, and No. 88, 1866) to this Society, we gave an account of some experiments which we had made upon the heating of a disk through rotation *in vacuo*. In these experiments the increase of radiation of the heated disk was observed by means of a delicate thermopile and galvanometer. Three aluminium disks of various thicknesses and one ebonite disk were used; and the results derived from the experiments were as follows:—

(1) The heating effect observed appeared to be independent of the density, and of the chemical constitution, of the residual air and vapour surrounding the disks.

(2) The quantity of heat developed under similar circumstances of rotation in three aluminium disks $\cdot 05$, $\cdot 0375$, $\cdot 025$ of an inch in thickness respectively appeared to be the same, inasmuch as the relative thermometric effect for these disks varied inversely as their thickness.

(3) Besides the heating effect alluded to in (1) and (2), there was found to be, when the vacuum had been recently made, a strictly temporary effect, sometimes in the direction of heat, sometimes in that of cold, owing probably to the condensation or evaporation of small quantities of aqueous vapour; but this effect was only noticeable during rotation, disappearing the moment the motion was stopped.

27. The experiments described in these communications were resumed in 1870. In the interval an addition had been made to the apparatus, in virtue of which an ordinary carbonic-acid vacuum might be subjected to the influence of a vessel containing potash allowed to open in it, and thus to absorb as much as possible of the remaining gas.

On May 4 a carbonic-acid vacuum was obtained by this means (pressure $0\cdot 05$ in.). A disk made of cartridge-paper, when made to rotate in this vacuum, gave a very perceptible result. Carbonic acid was then allowed to enter the vacuum until the pressure became $0\cdot 65$ in. The consequence of this increased pressure was in this instance an increase in the effect, which was probably of a permanent nature, inasmuch as it remained after three days. Unfortunately the exact increase was not noted; but it is believed that the heat-indication became about three times as great in consequence of the additional pressure.

28. On May 11 another carbonic-acid vacuum was made (pressure $0\cdot 12$ in.), and at the suggestion of Professor Maxwell a sulphuric-acid gauge was placed in the receiver. A rotation was then made; and the result of the rotation was a hardly perceptible rise in the sulphuric-acid

gauge. We may therefore imagine that the residual air was not greatly heated.

29. On May 16 another carbonic-acid vacuum was obtained (pressure 0.08 in.), and with an ebonite disk of about $\frac{1}{16}$ inch thickness, the heat-indication was 16. Carbonic acid was introduced until the pressure was 1.6 in.; but the indication was only 18. This result is in conformity with our previous experiments, in which an increase of pressure of the residual air produced little or no effect.

30. The ebonite disk was likewise tried in a carbonic-acid vacuum, pressure 0.04 in.; and also in one, pressure 0.02 in., which was the lowest we could obtain. The result of these experiments appeared to show that in all probability the cartridge-paper disk radiated more than twice as much as the thin ebonite disk. The experiments were put a stop to by a collapse of the glass receiver during rotation, fortunately without injuring any one present.

31. In June 1871 the experiments were resumed. In the mean time Mr. Beckley had fitted the apparatus with an arrangement working through a barometer-tube, by means of which, instead of trusting to radiation, the disk itself might, after rotation, be tapped by means of the pile, which could be brought up to it and then withdrawn. By this means a much larger effect might be obtained; and it became possible, by varying the adjustment, to find according to what law the heat effect varies with the distance from the centre.

32. These experiments were conducted in the following manner:—The disk was first of all tapped before rotation several times; at each tapping the momentary swing of the needle was recorded, and the mean of the readings was regarded as indicating the state of the disk with respect to heat.

The disk was next tapped after rotation, and the difference between the readings before and after was taken as indicating the change in the state of the disk produced by rotation. In the later experiments the disk before rotation was kept in slight motion in order to equalize any tendency to unequal heating of its various parts; but this was not done in the experiments of June 1871. In these experiments (June 1871) the disk was of brown paper, and the results obtained were as follows:—For a carbonic-acid vacuum (pressure 0.065 in.) a swing of 307 divisions was recorded, while for a hydrogen vacuum (pressure 0.150 in.) a swing of 281 divisions was recorded. Each result was the mean of three rotations.

33. The next experiments were made in January 1872. The galvanometer was one of Thomson's, but more adapted for battery currents, and hence not in a very delicate state for these experiments; the time of vibration of the needle was 3 seconds. The disk used was of ebonite (thickness about $\frac{1}{16}$ in.). The ebonite of this disk was completely black; and in this respect, as well as in being thinner, it differed from the ebonite disk first used in the radiation-experiments (art. 17). In the pre-

sent experiments the centre of the pile was made to tap the disk at a distance of 1.5 in. from the rim. The amount and velocity of rotation were represented by 30 turns of the handle, or 3750 turns of the disk, in about 40 seconds. The following results were obtained, a result representing on an average somewhat more than four rotations :—

	Pressure of residual gas, in inches.	Heat-indication for		
		Dry hydrogen.	Dry air.	Dry carbonic acid.
(A)	$\frac{9}{20}$	11.5	35.0	33.0
(B)	$\frac{3}{20}$	7.5	15.0	15.0

It would thus appear that the results derived by tapping are very different from the radiation-results, inasmuch as in the former the effect of the pressure and quality of the residual air is very apparent, while in the radiation-results it is hardly perceptible. A probable explanation of this will be given afterwards (art. 46); but in the mean time, in view of these results, it will be expedient to discuss them quite independently and by themselves, with the view of ascertaining whether they can best be explained by a gas-effect alone, or whether they likewise indicate a residual effect independent of gas.

34. With this object let us take $\frac{(A)+(B)}{2}$ as representing the *whole effect* at a pressure of $\frac{6}{20}$ in., due to whatever cause or causes. We thus obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Whole effect at $\frac{6}{20}$..	9.5	25.0	24.0

Again, let us suppose that (A)−(B) denotes the *gas-effect* for $\frac{6}{20}$ in., and we obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Gas-effect at $\frac{6}{20}$	4.0	20.0	18.0

Finally, let us regard as *unknown residual effect* the difference between the *whole effect* and the *gas-effect*, and we obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Residual effect	5.5	5.0	6.0

35. Similar experiments with the same galvanometer were made with a disk of cartridge-paper, of which the pores were filled with solid paraffin. And here we may mention that in all experiments with paper disks a small wooden attachment was placed at some little distance behind the disk and at the height of the pile, and against this the disk was pressed during tapping—care being taken that the disk did not touch it during motion, but only when it was pressed against it by bringing up the pile.

With this paper disk the amount and speed of motion were represented by 30 turns of the handle, or 3750 turns of the disk, in 30 seconds. The

results were as follows, a result representing on an average somewhat less than four rotations :—

	Pressure of residual gas, in inches.	Heat-indication for		
		Dry hydrogen.	Dry air.	Dry carbonic acid.
(A)	$\frac{9}{30}$	27·0	55·0	55·0
(B)	$\frac{3}{20}$	23·0	35·0	32·0

Treating these results in the same manner as those of the ebonite disk, we obtain :—

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Whole effect ($\frac{9}{30}$) ..	25·0	45·0	43·5
Gas-effect ($\frac{9}{30}$)	4·0	20·0	23·0
Residual effect	21·0	25·0	20·5

36. Now, if we suppose that there is only one effect due to gas, it follows :—

(α) That the proportion between the effects due to the various gases experimented on (and all of the same pressure) is nevertheless different for the two disks.

(β) That the proportion (for the same disk) between the effects due to the various gases experimented on is different according to the pressure.

If, however, we suppose that there are two effects, one of which is independent of the residual gas, we find :—

(α) That, as regards the *gas-effect*, the proportion between that due to the various gases is nearly the same for both disks. Thus in the ebonite disk we have 4, 20, 18, while in the paper disk we have 4, 20, 23 as representing the gas-effect for the various gases.

(β) That the *residual effect* in either disk is nearly the same for the various gases. Thus in the ebonite disk we have 5·5, 5·0, 6·0, while in the paper disk we have 21·0, 25·0, 20·5 as representing the residual effect for the various gases.

The results are thus much more simple on the hypothesis of two effects, one of these being independent of the residual gas, than on the hypothesis of only one effect.

37. It was next endeavoured to ascertain whether these two effects were differently influenced by a blind.

A linen blind was first used before the thin ebonite disk, which it cloaked during rotation, falling down afterwards. During rotation the blind did not appear to touch the disk. A delicate galvanometer was used for this experiment; and the time of vibration of the needle was 2 seconds. The rotation consisted of 30 turns of the handle in 40 seconds, as before. The following result was obtained (each number representing the mean of two observations) :—

	Without blind.	With blind.
$\frac{3}{20}$ of dry hydrogen	165	127
$\frac{3}{20}$ hydrogen + $\frac{9}{20}$ air	477	248

Thus the proportion between the two effects is greatly altered by the blind; for while the hydrogen effect is not much diminished, the other is reduced to little more than one half.

38. A chamois-leather blind was next used, which lay close to the disk during rotation. For all these experiments the time of vibration of the galvanometer-needle was 20 to 30 seconds, while the speed was the same as before. Also the disk was tapped immediately after rotation, and then again at the interval of one minute; and in order to obtain as many individual results as possible, the mean of these two tappings has been taken in the following comparison:—

	Without blind.	With blind.
$\frac{2}{30}$ of dry hydrogen .	53 { mean of 76 individual observations.	52 { mean of 22 individual observations.
$\frac{2}{30}$ hydrogen + $\frac{4}{30}$ air	217 { mean of 68 individual observations.	140 { mean of 16 individual observations.

Here also we find that the proportion between the two effects is greatly altered by the blind; so that while the hydrogen-effect is not much stopped, the other is diminished very considerably.

39. A set of experiments of 8 observations, each similar to those of arts. 33, 34, 35, were made with the blind on for the two gases, dry hydrogen and dry air. From these we obtain the following:—

	Pressure of residual gas, in inches.	Heat-indication for	
		Dry hydrogen.	Dry air.
Blind on	$\left\{ \frac{2}{30} \right.$	83	178
	$\left. \frac{4}{30} \right\}$	52	82

From which we deduce:—

Residual effect, blind on	36.5	34
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Again, from certain experiments with the blind off (each result being the mean of 6 observations), we obtain the following effect:—

For dry hydrogen	$\left\{ \begin{array}{l} (\frac{2}{30}) \\ (\frac{4}{30}) \end{array} \right.$	$\begin{array}{l} 88 \\ 50 \end{array}$
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and hence we find residual effect = 31.

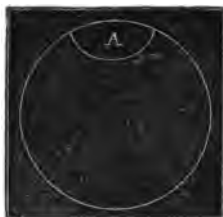
From all these results, allowing for errors of experiment, we may suppose that the residual effect is not much altered by a chamois-leather blind.

We ought to mention that the numbers of this article, similarly to those of art. 38, represent the mean of two tappings, the one taken immediately after rotation, and the other after the interval of one minute.

40. It was suggested to us by Prof. Helmholtz that it would be desirable to ascertain whether any difference was produced in the results by loading the disk on one side; for if these results were due to vibration, it might be supposed that they would be affected by this means.

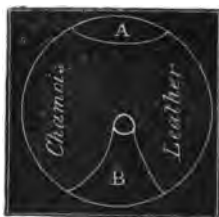
It will be seen by art. 39 that the residual effect obtained from a disk covered with chamois leather is approximately the same as that from an uncovered disk; this would appear to us to be against the vibration hypothesis.

In the following experiment the disk was covered with a chamois-leather blind with a segment cut out as underneath—



A mean of two sets of experiments (consisting of 16 separate observations, and embracing results immediately after rotation, as well as results one minute after) gave as the result of tapping in A with $\frac{3}{20}$ dry hydrogen 47, while a mean of 24 similar observations with the same atmosphere gave as the result of tapping an uncovered disk 53. Allowing for errors of experiment, we may conclude that this arrangement does not much influence the results.

41. The disk was next treated in the following manner:—



It was covered with a chamois leather blind tied into holes drilled in the disk, and having the above shape. The mean of 3 sets of experiments similar to those in art. 40, and consisting of 18 separate observations, gave as follows for an atmosphere of $\frac{3}{20}$ dry hydrogen, tapping in A 54, tapping in B 56, which two results are approximately equal to one another, the mean being 55.

The mean of 3 sets of experiments, consisting of 24 separate observations for an atmosphere of $\frac{3}{20}$ hydrogen, but with an uncovered disk, gave 53 as the heat-result. All these experiments apparently combine to prove that the result is not due to vibration.

42. When the atmosphere was $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air, we also found A and B approximately the same, A giving 181 while B gave 182; we have therefore not hesitated to combine together the results of tapping in this atmosphere, whether at A, or at B, or with an uncovered disk, in the comparison given in art. 38.

43. Our next experiments were made with the view of testing whether or not the two effects, the residual and the gas-effect, were resident in the same particles of the disk; and for this purpose the experiments made immediately after rotation were compared with those made one minute afterwards.

The experiments available for this purpose are so numerous that they can bear splitting into two portions, in each of which the same result is seen.

Thus we have for an atmosphere of $\frac{3}{20}$ dry hydrogen, and as the mean of 30 individual comparisons,

Effect at first : Effect one minute after :: 1.30 : 1 ;

also, as the mean of 22 individual comparisons, we obtain the proportion of 1.19 : 1, while as the mean of the whole we obtain 1.25 : 1.

Treating in a similar manner the observations made with an atmosphere of $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air, we obtain

As the mean of 25 comparisons 1.47 : 1,

As the mean of 21 comparisons 1.41 : 1,

while as the mean of the whole we obtain 1.44 : 1.

We therefore conclude that the residual effect is less diminished during the interval of one minute than the gas-effect.

44. We next made experiments with two aluminium disks .05 and .025 of an inch in thickness respectively. These disks were covered on both sides with a coating of lampblack applied by negative photographic varnish.

In both disks we found that with a vacuum of $\frac{3}{20}$ hyd. the effect one minute after rotation was greater than the effect immediately after rotation. Thus we have:—

For thick disk (mean of 4 observations) } Effect at first : Effect one minute after :: .85 : 1 ;

For thin disk (mean of 2 experiments, embracing 7 observations) } do. : do. :: .86 : 1.

But in cases where the gas-effect was considerable, this peculiarity disappeared. Thus we have for an atmosphere of $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air:—

For thick disk (mean of 2 observations) } Effect at first : Effect one minute after :: 1.05 : 1.

For thin disk (mean of 2 observations) } do. : do. :: 1.45 : 1.

Also, for an air-vacuum of $\frac{9}{20}$, we have

For thin disk (mean of 3 observations) } Effect at first : Effect one minute after :: 1.49 : 1.

In all the experiments with aluminium disks the needle of the galvanometer vibrated 20 times in 20 seconds, and the rotation consisted of 30 turns of the handle in 40 seconds.

Without pretending to explain all these experiments*, it must, we think, be concluded from them that there are two effects which are differently distributed over the particles of the disk.

45. The relation between the effect for $\frac{3}{20}$ hyd. and that for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air for the thin aluminium disk (galv. 20 in 20 seconds) is given as follows:—

$$\text{Effect for } \frac{3}{20} \text{ hyd. (mean of 22 observations)} : \left\{ \text{Effect for } \frac{3}{20} \text{ hyd.} + \frac{6}{20} \text{ air (mean of 10 observations)} \right\} :: 48 : 228.$$

In the preceding experiments the centre of the pile was at a distance (along the radius) of 1.5 in. from the rim of the disk. In the following experiment it was adjusted so as to be at a radial distance of 2.9 in. from the rim of the same disk. With this alteration and with the thin aluminium disk we obtained

$$\text{Effect for } \frac{3}{20} \text{ hyd. (mean of 7 observations)} : \left\{ \text{Effect for } \frac{3}{20} \text{ hyd.} + \frac{6}{20} \text{ air (mean of 9 observations)} \right\} :: 37 : 175.$$

We thus see that the effect for $\frac{3}{20}$ hyd. (which may be supposed to represent the residual effect) and that for $\frac{3}{20}$ hyd. + $\frac{6}{20}$ air (which may be supposed to represent the gas-effect) are both diminished in very nearly the same proportion, namely 100 : 77, by the above transference of the pile to a position nearer the centre of the disk. It would thus appear that the difference in distribution among the particles of the two effects brought to light in art. 44 cannot be explained by a difference from centre to rim, but would rather seem to be due to a difference in depth. If this appear to be improbable, it must be remembered that these experiments were not made on a naked metallic disk. These experiments would therefore appear to show that in an aluminium disk covered with varnish, as well as in a disk of ebonite, we may imagine the residual effect to be more deeply seated than the gas-effect.

46. We venture on the following as what appears to us to be the most probable explanation of the whole body of experiments, including those with radiation.

(1) There is a temporary heat or cold effect (art. 26) which may be supposed to arise in particles very slightly attached to the disk; this is radiated off chiefly during rotation, and probably does not greatly affect the disk afterwards.

(2) There is a surface gas-effect, which in an aluminium and even in an ebonite disk is conducted into the interior as it arises, so that it does

* May not this peculiarity of the aluminium disk in the hydrogen vacuum be due to some hygrometric surface-effect resident in the varnish? In one experiment, but only one, the peculiarity was absent.

not greatly radiate during rotation of the disk (art. 26). In a paper disk, however, which is formed of a badly conducting material loosely put together, part of the effect does escape as radiation during rotation (art. 27).

(3) There is a residual effect, which is more deeply seated than the gas-effect. And inasmuch as radiation takes place from a perceptible depth, this effect is much more influential than the gas-effect in increasing radiation after rotation. In the case of a paper disk, this deeply seated effect will be less diminished by radiation during rotation than the gas-effect, and therefore after rotation in such a disk we might expect the gas-effect to be peculiarly small (art. 35).

47. In the course of these experiments we have endeavoured to prove that this residual effect is not caused by vibration. The radiation-experiments with aluminium disks of three different thicknesses went, on the other hand, to show that it was of the nature of a surface-effect. This is confirmed by the results derived from tapping; for, in the first place, the experiments of art. 45 show that the two effects (the residual and the gas-effect) are probably distributed in the same proportion, going from the centre to the circumference of the disk. Again, taking the two disks of thickness $\cdot 05$ and $\cdot 025$ of an inch, we obtain the following results:—

	Effect for $\frac{1}{10}$ hyd.	Effect for $\frac{1}{10}$ hyd. + $\frac{1}{10}$ air.
Thin disk	48 (22 observations).	228 (10 observations).
Thick disk	29 (20 observations).	108 (10 observations).

Now, allowing for errors of experiment, we see that the residual, as well as the gas-effect, is reduced to about one half for the thick disk.

Again, an experiment of a similar nature gave the effect for $\frac{1}{10}$ hyd. in an ebonite disk of $\frac{1}{10}$ in. in thickness = 33, against a result = 55 for the thin ebonite disk. Unfortunately it was omitted to make a comparison with these two disks for the gas-effect; nevertheless these results are all in favour of the residual effect being a surface-effect.

48. It might be well to make one remark regarding these experiments. They are not like the radiation-experiments, which required an extremely delicate instrument in order to give a sensible effect. But, on the other hand, the effect obtained by tapping being that due to the mere surface of the disk, is liable to be altered by any thing which affects the surface of the disk. We have come to the conclusion that in such experiments it is unadvisable to use a porous hygrometric surface, such as that of paper, not having its pores filled with paraffin or some other similar substance. It is likewise desirable that all parts of the apparatus should be as nearly as possible of the same temperature; indeed we suspect that some experiments made during some very peculiar summer weather were influenced by a want of temperature-equilibrium between the various parts of the apparatus, the result appearing to be that the gas-effect for the ebonite disk was abnormally large.

49. Our conclusion from the evidence before us is, that the residual effect is a surface-effect more deeply seated than the gas-effect, but distributed outwards from the centre to the circumference, very much in the same manner as the gas-effect. The residual effect likewise appears able to penetrate a chamois-leather blind without any perceptible diminution. We regard these conclusions as preliminary, and shall endeavour in our future experiments to procure additional evidence of these properties of the residual effect, as well as to obtain new facts regarding it. In the mean time, as the subject is one of interest, and has been already too long delayed, we have not hesitated to bring these results before the notice of the Royal Society.

In concluding we would desire to express our thanks to Mr. F. Kingdon for his assistance to us in many of these experiments.

V. "On the Extension of the Numerical Value of π ." By WILLIAM SHANKS, Houghton-le-Spring, Durham. Communicated by Prof. G. G. STOKES, Sec. R.S. Received April 16, 1873.

In the 'Messenger of Mathematics' for Dec. 1872, J. W. L. Glaisher, Esq., has given some very interesting particulars regarding the calculation of π , in the justness of which the author generally concurs. He, however, differs from him as to the comparative merits of Van Ceulen, who, in the early part of the seventeenth century, calculated π to 36 decimals. Hutton's formula also, given in the 'Messenger,' appears, notwithstanding Hutton's own opinion, to be not so well adapted for extensive computation as Machin's, which the author has used on the present as well as former occasions, regarding it as the best yet found.

The values of $\tan^{-1} \frac{1}{2}$ and of $\tan^{-1} \frac{1}{239}$ are each given below to 709, and the value of π to 707 decimals. It will be observed that a few figures in the values of $\tan^{-1} \frac{1}{2}$ and of π , published in 1853, were erroneous. The author detected the error quite recently, and has corrected it. The values of each term of the two series in $\frac{\pi}{4} = 4 \tan^{-1} \frac{1}{2} - \tan^{-1} \frac{1}{239}$, are far too bulky to be given *in extenso*: fortunately, but few would care to see them!

It may here be stated that Prof. Richter, of Elbing, found π to 500 decimals in the year 1853—all of which agree with the author's, published early in the same year.

The Society adjourned over Ascension Day to Thursday, May 29.*

Value of $\tan^{-1} \frac{1}{2}$.

19739 55598 49880 75837 00497 65194 79029 34475 85103 78785 21015 17688
 94024 10339 69977 24378 57326 97828 03728 80441 12628 11807 36913 60104
 45647 98867 94239 35574 75654 95216 30327 00522 10747 00156 45015 56006
 12861 85526 63325 73186 92806 64389 68061 89528 40582 59311 24251 61329
 73139 93397 11323 35378 21796 08417 66483 10525 47303 96657 25650 48887
 81553 09384 29057 93116 95934 19285 18063 64919 69751 94017 08560 94952
 73686 73738 50840 08123 67856 15800 93298 22514 02324 66755 49211 02670
 45743 78815 47483 90799 78985 02007 52236 96837 96139 22783 54193 25572
 23284 13846 47744 13529 09705 46512 24383 02697 56051 83776 17781 64242
 33783 03370 18192 64880 28277 68611 91509 85606 75901 21359 85563 63034
 32100 56649 97826 76360 88711 52327 56610 84900 93773 38023 19504 70687
 65729 38513 59243 19759 37947 36057 50636 20935 07853 2833 &c.

Value of $\tan^{-1} \frac{1}{2\sqrt{3}}$.

00418 40760 02074 72386 45382 14959 28545 27410 48065 30763 19508 27019
 61288 71817 78341 42289 32737 82605 81362 29094 54975 45066 64448 63756
 05245 83947 89311 86505 89221 28833 09280 08462 71962 33077 33759 47634
 60331 84734 14570 33198 60154 54814 80599 24498 30211 46039 12539 49527
 60779 68815 58881 27339 78533 46518 04574 25481 35867 46447 51979 10232
 83097 70020 64652 82763 46532 96910 48183 86543 56078 91959 14512 32220
 94463 68627 66155 20831 67964 26465 74655 11032 51034 35262 82445 12693
 55670 49968 44452 47904 33177 28393 07086 31401 93869 51950 37058 64107
 70855 85540 45223 55388 14237 67708 36515 69182 52702 00229 30895 44950
 04358 54409 34496 44014 24187 24950 92283 86239 54553 33565 16494 21220
 06852 38821 94006 45849 29313 23886 73467 64889 18731 81682 83021 21101
 37897 11546 96191 84692 18237 33903 04682 04140 79985 6684 &c.

Value of $\pi=3$.

14159 26535 89793 23846 26433 83279 50288 41971 69399 37510 58209 74944
 59230 78164 06286 20899 86280 34825 34211 70679 82148 08651 32823 06647
 09384 46095 50582 23172 53594 08128 48111 74502 84102 70193 85211 05559
 64462 29489 54930 38196 44288 10975 66593 34461 28475 64823 37867 83165
 27120 19091 45648 56692 34603 48610 45432 66482 13393 60726 02491 41273
 72458 70066 06315 58817 48815 20920 96282 92540 91715 36436 78925 90360
 01133 05305 48820 46652 13841 46951 94151 16094 33057 27036 57595 91953
 09218 61173 81932 61179 31051 18548 07446 23798 34749 56735 18857 52724
 89122 79381 83011 94912 98336 73362 44193 66430 86021 39501 60924 48077
 23094 36285 53096 62027 55693 97986 95022 24749 96206 07497 03041 23668
 86199 51100 89202 38377 02131 41694 11902 98858 25446 81639 79990 46597
 00081 70029 63123 77381 34208 41307 91451 18398 05709 85 &c.

April 14, 1873.

*Presents received April 24, 1873.***Transactions.**

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“Further Observations on the Temperature at which *Bacteria*, *Vibriones*, and their Supposed Germs are killed when exposed to Heat in a moist state; and on the Causes of Putrefaction and Fermentation.” By H. CHARLTON BASTIAN, M.A., M.D., F.R.S., Professor of Pathological Anatomy in University College, London. Received March 25, Read May 1, 1873.

Whilst a heat of 140° F. (60° C.) appears to be destructive to *Bacteria*, *Vibriones*, and their supposed germs in a neutral saline solution, a heat of 149° or of 158° F. is often necessary to prevent the occurrence of putrefaction in the inoculated fluids when specimens of organic infusions are employed. What is the reason of this difference? Is it owing to the fact that living organisms are enabled to withstand the destructive influence of heat better in such fluids than when immersed in neutral saline solutions? At first sight it might seem that this was the conclusion to be drawn. We must not, however, rest satisfied with mere superficial considerations.

The problem is an interesting one; yet it should be clearly understood that its solution, whatever it may be, cannot in the least affect the validity of the conclusion arrived at in my last paper, viz. that living matter is certainly capable of arising *de novo*. We were enabled to arrive at the conclusion above mentioned regarding Archebiosis by starting with the undoubted fact that a heat of 158° F. reduces to a state of potential death all the *Bacteria*, *Vibriones*, and their supposed germs which an organic infusion may contain. The inquiry upon which I now propose to enter, therefore, touching the degree of heat *below this point* which may suffice to kill such organisms and their supposed germs in an organic infusion, and touching the cause of the delayed putrefaction apt to take place in inoculated organic infusions which have been heated to temperatures above 140° and below 158° F., is one lying altogether outside the chain of fact and inference by which the occurrence of Archebiosis is proved.

It seems to me that the solution of the problems which form the subject of the present communication can only be safely attempted by keeping constantly before our minds two main considerations:—

Thus, in the experiments whose results it is now our object to endeavour to explain, the fluids have been inoculated with a compound consisting partly (*a*) of living units, and partly (*b*) of a drop of a solution of organic matter in a state of molecular change; so that in many cases where putrefaction has been initiated after the inoculating compound has been heated to certain temperatures, there is the possibility that this process of putrefaction may have been induced (in spite of the death of the organisms and their germs) owing to the influence of *b*, the dissolved organic matter of the inoculating compound; that is to say

the heat to which the mixture has been exposed may have been adequate to kill all the living units entering into the inoculating compound, although it may not have been sufficient to prevent its not-living organic matter acting as a ferment upon the infusion*.

And there are, I think, the very best reasons for concluding that in all the cases in which turbidity has occurred after the organic mixtures have been subjected to a heat of 140° F. (60° C.) and upwards, this turbidity has been due, not to the survival of the living units, but rather to the fact that the mere dead organic matter of the inoculating compound has acted upon the more unstable organic infusions in a way which it was not able to do upon the boiled saline fluids.

In order more fully to explain the grounds upon which this conclusion is based, it will now be necessary to recast the results of the 102 inoculation experiments recorded in my last communication†. They require to be thrown into a new tabular form, in order to show how the results differed amongst themselves when organic infusions of different strengths were employed. The consideration of this aspect of the question was purposely delayed, in order to avoid the introduction of unnecessary complications into my last communication, seeing that the conclusion which I then sought to establish was in no way affected by these facts.

Neutral Hay-Infusion.

Infusion of sp. gr. 1005.				Infusion of sp. gr. 1002.		
Temperature.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.
122° F. (50° C.)	} 1	24 hrs.	Turbid.
131° F.		48 hrs.	All turbid.	1	48 hrs.	Turbid.
140° F. (60° C.)	} 7 {	1 in 48 hrs.	} All turbid.	2 {	1 in 3 days.	} All turbid.
149° F.		6 in 60 hrs.		4 {	1 in 8 days. 2 in 5 days. 1 in 8 days.	
158° F. (70° C.)	}	15	All clear.
167° F.		4	All clear.
176° F. (80° C.)	} 12	All clear.

* See 'The Beginnings of Life,' vol. ii. p. 2.

† See Proceedings of the Royal Society, No. 143, p. 230.

Acid Turnip-Infusion.

Infusion of sp. gr. 1008.				Infusion of sp. gr. 1005.		
Temperature.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.
122° F. 131° F. 5 24 hrs. All turbid. 2 48 hrs. All turbid.
140° F.	6	40 hrs.	All turbid.	6	{ 4 in 3 days. 2 in 4 days. 1 in 3 days. 1 in 7 days. 2 in 8 days.	{ All turbid. Four turbid. Three clear.
149° F.	3	5 days.	All turbid.	7		
158° F.	17	All clear.
167° F.	4	All clear.
176° F.

Reference will be made to these Tables in the setting forth of my reasons for the conclusion that the more or less delayed putrefaction which takes place in inoculated organic infusions raised to the temperature of 140° F., and to other degrees of heat above this point, is due to the influence of the not-living ingredient (b) of the inoculating compound. These reasons are the following:—

1. Because the turbidity which has occurred in inoculated organic infusions that have been subjected to a temperature of 140° F. has always manifested itself appreciably later, and advanced much more slowly than in similar mixtures which had not been heated above 131° F.; whilst it has commenced even later, and progressed still more slowly, when occurring in mixtures previously heated to 149° F. Such facts might be accounted for by the supposition that exposure in these organic fluids to the slightly higher temperature suffices to retard the rate of growth and multiplication of the living units of the inoculating compound, although the facts are equally explicable upon the supposition that the later and less energetic putrefactions are due to the sole influence of the mere organic matter of the inoculating compound.

2. So far as the evidence embodied in the Tables goes, it tends to show that the more unstable different specimens of similar infusions are (that is, the stronger they are), the more rapidly and frequently does late turbidity ensue, and the more this late turbidity approaches, both in time of onset and in rate of increase, to that which occurs when inoculated infusions are not heated to more than 131° F.—when both living and not-living elements of the inoculating compound act conjointly as ferments. Such facts show quite clearly that where the intrinsic or predisposing causes of change are strong, there less potent exciting agencies are more readily capable of coming into play; but they still do

not enable us to decide whether the exciting cause of this delayed turbidity is in part the living element whose vitality and rate of reproduction has been lowered by the heat, or whether the effects are wholly attributable to the mere organic matter of the inoculating compound.

So far, therefore, we have concomitant variations which are equally compatible with either hypothesis. But it will be found that each of the three succeeding arguments speaks more and more plainly against the possible influence of the living element, and in favour of the action of the organic matter of the inoculating compound, as an efficient exciting cause of the delayed putrefactions occurring in the cases in question.

3. As stated in my last communication *, when single drops of slightly turbid infusions of hay or turnip previously heated to 140° F. are mounted and securely cemented as microscopical specimens, no increase of turbidity takes place, although drops of similar infusions heated only to 122° F. do notably increase in turbidity (owing to the multiplication of *Bacteria*) when mounted in a similar manner. Under such restrictive conditions as these, in fact, a drop of an inoculated and previously heated organic infusion behaves in precisely the same manner as a drop of a similarly treated ammoniac-tartrate solution. In each case, when heated to 140° F., turbidity does not occur, apparently because there are no living units to multiply, and because in these mere thin films of fluid dead ferments are as incapable of operating upon the organic fluids as they are upon the ammoniac-tartrate solutions.

4. Because, in the case of the inoculation of fluids which are not easily amenable to the influence of dead ferments, such as a solution containing ammoniac tartrate and sodic phosphate, this delayed turbidity does not occur at all. Such inoculated fluids become rapidly turbid when heated to 131° F., though they remain clear after a brief exposure to a temperature of 140° F. When the living units in the inoculating compound are killed, there is nothing left to induce turbidity in such solutions. The mere fact that these fluids do not undergo change when exposed to the air proves conclusively that they are very slightly amenable to the influence of the ordinary dead organic particles and fragments with which the atmosphere abounds. The absence of delayed turbidity in these fluids serves, therefore, to throw much light upon the cause of its occurrence in the organic infusions.

5. And, lastly, I can adduce crucial evidence supplied by the "Method of Difference," speaking with its accustomed clearness. Two portions of the same hay- or turnip-infusion can be inoculated in such a manner as to supply us with the information we require. In the one case we may employ a drop of a turbid ammoniac-tartrate solution previously heated to 140° F., in which, therefore, the living units would certainly be killed; whilst in the other we may add an unheated drop of the same turbid saline solution to the organic fluid, and then heat this mixture also to

* *Loc. cit.* p. 228.

the temperature of 140° F. The comparative behaviour of these two inoculated fluids (placed, in the ordinary manner, in previously boiled corked phials) should be capable of showing us whether the living elements of the inoculating compound were able to survive when heated in the organic infusion. If they did survive, the fluids inoculated in this manner ought to undergo putrefaction earlier and more rapidly than those inoculated with the drop of turbid fluid, in which we know that the *Bacteria*, *Vibriones*, and their supposed germs would have been reduced to a state of potential death. With the view of settling this question, therefore, the following experiments were made:—

Description of Experiments.	Results.	Inferences.
A. Boiled ammonio-tartrate solution, inoculated with an unheated drop of a similar solution turbid with <i>Bacteria</i> &c.	Turbid in 40 hrs.	That boiled ammonio-tartrate solution is a fluid inoculable by living <i>Bacteria</i> &c., and favourable for their growth and rapid multiplication.
B. Boiled ammonio-tartrate solution, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Clear at expiration of 8th day.	That <i>Bacteria</i> , <i>Vibriones</i> , and their supposed germs are either killed or deprived of all power of multiplication when heated to 140° F. in this fluid.
C. Boiled turnip- and hay-infusions, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	The precisely similar behaviour of the turnip- and hay-infusions of series C and series D respectively shows that the <i>Bacteria</i> , <i>Vibriones</i> , and their supposed germs are as inoperative in series D as they are known to be in series C; whilst the behaviour of the hay-infusions shows that they are little amenable to the influence of the drop of the saline fluid when its living units are killed.
D. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated turbid saline solution, the inoculated fluid being subsequently heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	
E. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated saline solution, the inoculated fluid being subsequently heated to 131° F.	Turnip-infusions turbid in 28 hrs. Hay-infusions turbid in 38 hrs.	
		Shows that a heat of 131° F. is not sufficient to kill <i>Bacteria</i> , <i>Vibriones</i> , and their supposed germs in organic infusions, and, again, that turnip-infusions are more rapidly influenced by such an inoculating agent than some hay-infusions*.

No experiments could speak more decisively. Those of series B show that *Bacteria*, *Vibriones*, and their supposed germs are either actually or potentially killed when heated to 140° F. in the neutral saline fluid,

* These experiments of series C, D, and E were many times repeated with specimens of the same turnip- and hay-infusions, the specific gravity of the former being about 1008 and that of the latter 1005. Different specimens of hay especially vary so much that it becomes absolutely essential to use portions of the same infusion for the comparative experiments of these different series.

which the experiments of series A show to be eminently favourable for their growth and reproduction. Being certain, therefore, that the living units are killed in the drops with which the fluids of series C were inoculated (because they were drops of the same fluid as was employed in series B), we may be equally certain that the turbidity and putrefaction which did ensue in the turnip-solutions of series C were due to the influence of the mere dead constituents of these drops of the turbid saline fluid; whilst, seeing that the behaviour of the fluids of series D was precisely similar to those of series C, we have a perfect right to infer that this series of fluids (D) was as devoid of living units as those of C are known to be—that is, that *Bacteria*, *Vibriones*, and their supposed germs are killed by the temperature of 140° F. in organic fluids, just as they are in saline fluids, although, as shown by the experiments of series E, they do not succumb to a heat of 131° F. These experiments of series C and D further illustrate the different degrees of amenability of different organic fluids to the same dead ferments; whilst the comparison of the results with the hay-infusions of series C and D with those previously cited (in which the inoculating compound was a drop of an organic infusion heated to the same temperature of 140° F.) will illustrate the different influence of dissimilar dead ferments upon infusions of the same kind.

The evidence now in our possession shows, therefore, that whilst the temperature at which living ferments cease to be operative varies within very narrow limits (131°–140° F.)*, that which destroys the virtues of not-living ferments varies within much wider limits, and depends not only upon the amount of heat employed, but also upon the nature of the putrescible or fermentable liquid to which such ferment is added, in conjunction with the degree of heat and other conditions to which the mixture is subsequently exposed†. Here, therefore, we have evidence as to the existence of a most important difference between living and not-living ferments, which has always been either unrecognized or more or less deliberately ignored by M. Pasteur and his followers‡. This differ-

* Liebig has proved that a temperature of 140° F. kills *Torula*, and always suffices to arrest a process of fermentation taking place under their influence in a sugar solution. *Torula* heated in water to 140° F. also fail to initiate fermentation in a sugar solution. I have also found that an exposure to a temperature of 131° F. for five minutes always suffices to destroy the life of Desmids, Euglenæ, Amœbæ, Monads, Ciliated Infusoria, Rotifers, Nematoids, and other organisms contained in specimens of pond-water. All these lower organisms seem to be destroyed at about the same temperature, as might have been expected from the fundamental relationship which must exist between these several varieties of the one substance—living matter.

† See 'The Beginnings of Life,' vol. i. p. 437.

‡ See, for instance, all M. Pasteur's celebrated experiments in which he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," as recorded in chaps. iv. & v. of his memoir in 'Ann. de Chimie et de Physique,' 1862. M. Pasteur was engaged in an investigation one of the avowed objects of which was to determine whether fermentation could or could not take place without the intervention of

ence is, moreover, thoroughly in accordance with the broad physico-chemical theory of fermentation which has been so ably expounded by Baron Liebig and others, and the truth of which may now be regarded as definitely established. According to this theory "living" matter, as a ferment, would take rank merely as a chemical compound having a tolerably definite constitution; and this, we might reasonably infer, would, like other chemical compounds, be endowed with definite properties—and amongst others that of being decomposed or radically altered by exposure to a certain amount of heat. Looked at also from this essentially chemical point of view, it would be only reasonable to expect that the molecular movements of living ferments with a lowered vitality might not be more marked or energetic than those which many not-living organic substances are apt to undergo; and this being the case, we might expect that there would often be a great practical difficulty in ascertaining whether a ferment belonging to the arbitrary and artificial (though, in a sense, justifiable and natural) category of "living" things had or had not been in operation.

It has, moreover, been most unmistakably proved that the limits of vital resistance to heat which *Bacteria*, *Vibriones*, and their supposed germs are capable of displaying are essentially the same in the three type fluids which I have employed—that is, in a weak saline fluid, in a neutral organic infusion, and in an acid organic infusion. No evidence exists really tending to show that these organisms or their germs are capable of withstanding the effects of heat better in one of such fluids than in another. We may therefore safely affirm that M. Pasteur never had any valid evidence in support of his conclusion that the germs of *Bacteria* and *Vibriones* can resist heat better in neutral or slightly alkaline solutions than in slightly acid mixtures. The experimental results which led him to arrive at such a conclusion were not logically capable of receiving any such interpretation, whilst they can be legitimately accounted for in accordance with the broader physico-chemical theory of fermentation, the truth of which has now been established*. We may also safely affirm that M. Pasteur's more specific statement, to the effect that

living organisms, which M. Pasteur held (in opposition to many other chemists) to be the only true ferments. In his inoculating compound (dust filtered from the atmosphere), there was, as M. Pasteur was fully aware, a large amount of what his scientific opponents considered not-living ferment, whilst possibly there existed a certain number of living ferments. In explaining the results of his experiments, however, M. Pasteur and others thought he was pursuing a logical and scientific method when he attributed these results to the action of the possibly existing element of the inoculating compound, whilst he ignored altogether the other element which was certainly present in comparatively large quantity, and the testing of whose efficacy was the ostensible object of his research.

* I attempted to show, nearly three years ago (see 'Nature,' July 14, 1870, pp. 224–228), that the differences which M. Pasteur ascribed to differences of vital resistance of organisms in particular fluids were just as explicable in accordance with the physico-chemical theory of fermentation, by reference to the different degrees of fermentability of the several fluids.

the germs of some *Bacteria* and *Vibriones* are capable of resisting the influence of a heat of 212° F. when in the moist state, though they are killed by a temperature of 230° F., was a conclusion altogether unwarranted by the evidence which he adduced. Finding that certain fluids treated after the manner introduced by Schwann always remained quite devoid of living organisms, M. Pasteur very legitimately concluded that preexisting organisms and germs had been killed during the boiling of the liquid; but finding that when a little powdered chalk was added to fluids of the same kind (which in all other respects were treated in a similar manner) living organisms were after a time invariably found to appear, although they as invariably failed to appear when the same fluids were heated to a temperature of 230° F. (110° C.), two equally legitimate provisional conclusions were open to M. Pasteur in explanation of these facts. What did M. Pasteur do? Following the same method as he had formerly employed*, he again ignored one of the equally possible interpretations, and unsuccessfully attempted to prove, by a repetition of similar reasoning†, that the different results in the two series of experiments were due to the fact that the germs of *Bacteria* and *Vibriones* which had been killed by the temperature of 212° F. in the first series, were not killed by this temperature in the second series (in which a slightly alkaline fluid had been employed), although they were destroyed by the higher temperature of 230° F. Thus results which were due to the action of not-living ferments were ascribed to living ferments, and the possible action of not-living ferments was ignored, although, as I have said before, the ostensible object of M. Pasteur's researches was to inquire into the relative importance of not-living and living ferments, or whether, in fact, "dead" substances (in the ordinary acceptance of the word) could act as ferments.

When viewed from the stand-point of the physico-chemical theory of fermentation, the apparently contradictory results arrived at by the same experimenter at different times or by different experimenters, in this line of research, cease to be the inexplicable puzzle which they must always appear to those who place implicit faith in the narrower and too exclusive "vital" theory of fermentation advocated by M. Pasteur and his followers.

My investigations have convinced me that, with regard to degree of fermentability, the various fermentable fluids and mixtures are divisible into three distinct subclasses:—

I. There are what may be called self-fermentable fluids or mixtures—that is, fluids or mixtures which, after exposure to a temperature of 212° F. or higher, are still capable of undergoing fermentative changes without the addition of less-heated matter, either not-living or living. The changes occurring in these self-fermentable fluids (in which preexisting living things have been killed), when strictly protected from contact with adventitious particles, vary in rapidity and in intensity from the

* See note ‡ on page 330.

† See Ann. de Chim. et de Phys. 1862, pp. 60–65.

highest to the very lowest degrees of fermentability. These gradations are dependent principally upon the nature of the fluids or mixtures employed, and upon the degree of heat to which they have been submitted, though partly also to the temperature, pressure, presence or absence of filtered air, and degree of light to which the mixtures are subsequently exposed. For the sake of convenience, these gradations may be ranged into several distinct groups, though of course any such divisions as I am now about to sketch are purely artificial and are connected with one another in nature by innumerable transitions.

Nature of Fluids.

A. Turnip-infusion with cheese, turnip-infusion neutralized by liquor potassæ, ordinary turnip-infusion, strong hay-infusion, &c.

B. Turnip-infusion neutralized by liquor potassæ, ordinary turnip-infusion, ordinary hay-infusion, &c.

C. Beer-wort*, &c.

D. Weak hay-infusions, urine, solutions containing ammoniac carbonate and sodic phosphate with minute organic impurities, &c.

E. Weak hay-infusions, urine, solutions containing ammoniac-citrate of iron and minute organic impurities†, &c.

F. Solutions of ammoniac tartrate and sodic phosphate with minute organic impurities, &c.

Nature of Results.

Within two to four days marked turbidity, owing to the appearance of swarms of *Bacteria* and *Vibriones*. Fluids more or less fetid. (*Putrefaction*.)

No uniform turbidity, but growth of flocculi in a more or less clear liquid. After a time the flocculi (composed of aggregated *Bacteria* and *Vibriones*) gradually subside, and the activity of the process ceases. Fluids either fetid or having a mere sour odour.

Fluids which become more or less uniformly and rapidly turbid, owing to the appearance of swarms of *Torule*.

Do not become visibly turbid or produce visible flocculi, although on microscopical examination they may be found to contain living *Bacteria* pretty uniformly distributed, but in comparatively small quantities. The odour is often not more appreciably altered than the clearness of such solutions.

Same as in the last group‡, though after weeks or months a dirty-looking sedimentary matter slowly accumulates at the bottom of the flask, which on microscopical examination is found to be composed partly of *Bacteria* with *Vibriones* and *Leptothrix*, and partly of *Torule* or more thick-walled fungus-germs.

Same as in the last group, only the dirty sedimentary matter which accumulates never contains either *Bacteria*, *Vibriones*, or *Leptothrix*. Living *Torule* and thick-walled fungus-germs in various stages of formation are frequently met with, and also, occasionally, a mycelium resulting from the development of some of these bodies.

* I have had no experience with such a fluid myself. M. Pouchet's observations were, however, most striking on this subject (see his 'Nouvelles Experiences,' Paris, 1864, p. 190).

† In solutions containing iron, green organisms may subsequently be found (see 'Beginnings of Life,' vol. ii. p. 157).

‡ This, in fact, is in many cases the kind of change which the fluids last described ultimately undergo.

Nature of Fluids.

G. Weak or strongly acid infusions, and also many saline solutions containing organic impurities.

Nature of Results.

May remain permanently barren, and never show any traces of organisms, either dead or living*.

II. To the second subclass belong fluids which, after exposure to a temperature of 212° F. or higher, may be kept clear or apparently unaltered so long as they are shut off from contact with unheated atmospheric or other organic particles, but which do undergo putrefaction, or more or less marked fermentation, soon after they are brought into contact even with mere not-living organic matter.

The experiments recorded in this communication have most conclusively proved the efficacy of not-living organic matter as a ferment or inciter of change in previously barren fluids. And combining the knowledge derived from these experiments with that which we now possess concerning the absence of living *Bacteria*, *Vibriones*, and their germs in the air, together with the known prevalence of minute organic particles and fragments of various kinds, the explanation of M. Pasteur's celebrated experiments in which he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," becomes quite easy and legitimate without having recourse to the hypothesis of Panspermism†. Now, also, are we enabled to understand all the apparent inconsistencies of those experiments in which previously boiled fluids have been exposed to the ordinary air of different localities, and have then been resealed. If many specimens of these fluids remained unchanged, whilst others, after a few days, swarmed with *Bacteria* and *Vibriones*, we may now very safely attribute these previously puzzling results to the comparative absence or presence of organic fragments in the particular volumes of air which chanced to get into the flasks, and to the different nature of the fluids employed by different experimenters‡.

* See many negative experiments recorded in 'The Beginnings of Life,' vol. i. ch. xi. Mr. W. N. Hartley has laboured very industriously to disprove something which I never asserted (see Proceedings of Royal Society, vol. xx. p. 140). In my early paper in 'Nature' I expressly stated that organic impurities were always present in the saline solutions which I employed; and, as may be seen by the note appended to the conclusions of that paper, I never claimed to have established that living organisms could appear in saline solutions free from traces of organic impurity. Mr. Hartley did attempt to work with approximately pure saline solutions, and in other respects also the conditions of his experiments differed so much from mine, that the results which he obtained could not possibly be considered to disprove what I had previously stated. Some of his flasks were heated to 180° C., a temperature about which I had said nothing; and whilst his organic infusions were too weak, some of his saline solutions were too concentrated, though the strengths of others were not given at all. Fluids were also employed (such as urine, heated to 130° C.) which I had not made use of, and which I should not have thought of experimenting with.

† See the experiments before alluded to, which are recorded in chap. iv. & v. of his Memoir.

‡ See M. Pasteur's Memoir, chap. vii., and also Compt. Rend. Nov. 5, 1860. See

Many of the fluids which habitually remain clear after a previous ebullition in flasks whose necks have been plugged with cotton-wool, many times bent, or hermetically sealed after the entry of calcined air, or when enclosed in vessels which are completely full (in Gruithuisen's fashion), belong to this subclass. In other cases, however (as in many of those instances where urine or hay- or turnip-infusions have been employed), those who do not content themselves with a mere naked-eye inspection of the apparently pure fluids would find on microscopical examination of the sediment that such fluids were to a low degree self-fermentable—that they correspond, in fact, with group E of the last subclass*; whilst, in addition, my researches have shown that many of such fluids are capable of being rendered self-fermentable to a marked degree, if, instead of subjecting them to contact with calcined air or variously filtered air, its reflux after ebullition is altogether prevented by hermetically sealing the neck of the flask during ebullition. Operating in this way, I have repeatedly found that fluids freed from the pressure of air and from its influence altogether become to a marked extent self-fermentable, although the same fluids exposed to filtered or calcined air under ordinary atmospheric pressure remain unaltered and barren, or at most exhibit the very low degree of fermentability referred to as characterizing group E†. But just as amongst the self-fermentable fluids we find there are some which only engender *Torula* or other allied fungus-germs, so now we find that some previously boiled fluids, even when fully exposed to the air, swarm only with *Torula*. Those exciting agents derived from the atmosphere which, with one set of fluids, initiate changes leading to the evolution of *Bacteria*, with another set lead only to the evolution of *Torula*. And whilst telling us that the *Bacteria* which appear in previously barren fluids after exposure to air are not due to their contamination with germs of *Bacteria*, some observers would have us conclude that the *Torula* which appear in other previously barren fluids after a similar exposure are the products of preexisting aerial germs of such organisms. This conclusion, however, cannot readily be accepted in the face of the evidence derived from the closed-flask experiments with self-fermentable fluids of the lowest degree‡. Such experiments, in fact, render the hypothesis as

also a record of other experiments made with the air of alpine regions by MM. Pouchet, Joly, and Musset, in Compt. Rend. Sept. 21, 1863.

* Other fluids richer in organic matter or otherwise more favourably endowed, instead of presenting this low degree of self-fermentability, are notably prone to undergo change when we attempt to preserve them in the manner described, although such modes of preparation do suffice for preserving so many fluids. This has been fully admitted by Schroeder and Dusch, Schwann, Pasteur, and others.

† In illustration of this statement see 'The Beginnings of Life,' Appendix C, Exps. viii., ix., xiv., xv., xviii., xx., xxvi., xxx., xxxiii., and xxxvi.

‡ The only evidence in favour of such a conclusion is not one jot more conclusive than that which was formerly adduced in favour of the universal prevalence of *Bacteria*-germs in the air.

to the widespread distribution of aërial germs of *Torula* wholly unnecessary, by showing that certain fluids, by reason of certain intrinsic peculiarities, when they undergo fermentation give rise to *Torula* only. We are thus led to conclude that whilst some fluids are capable of engendering both kinds of organisms, others tend only to produce one or other of them—whether the fluids are contained in closed flasks or in open vessels exposed to the incidence of atmospheric particles. I have more than once seen nothing but *Torula* appear in an infusion of turnip exposed to the air after it had been heated in a closed tube to a temperature of 293° F. for twenty minutes, and I have once seen the same thing occur in an unheated infusion of turnip exposed to the air, though on all other occasions such infusions have swarmed only with *Bacteria* and *Vibriones*. On the other hand, a boiled ammoniac-tartrate solution exposed to the air, though protected from an excess of atmospheric particles (for the advent of a large number of these might in some cases incite putrefaction), is never found to contain *Bacteria*; the fluid continues clear, though a sediment gradually accumulates at the bottom of the flask, amongst which *Torula* and other fungus-germs are constantly to be found—more numerous though otherwise very similar to those which are to be met with in flasks closed during ebullition, or in others to which only filtered air is admitted. Although *Torula* only appear in such fluids, they continue all the time to be eminently inoculable by *Bacteria*; and, again, when the *Torula* begin to decay they are apt to incite a more or less manifest putrefaction, during which the fluid gradually becomes turbid with *Bacteria*. It is, in fact, a general rule that putrefaction is apt to supervene upon a fermentation of a more smouldering type.

III. In the third subclass I include fluids which, after exposure to 212° F. or higher temperatures, are unable, either alone or under the influence of ordinary atmospheric particles or fragments, to undergo putrefaction, although such a process can invariably be initiated by bringing the fluids into contact with living ferments. As examples of such fluids, I may cite the neutral saline solution to which I have so often referred and that known as Pasteur's solution. Other fluids of the same kind have lately been referred to by Professor Huizinga*. The fact that certain fluids cannot be made to undergo putrefaction by the influence of dead organic particles, although they become at once amenable to the influence of living units, unmistakably shows the superior potency of living ferments; their action has, moreover, invariably proved to be certain and inevitable in all the cases in which they were known to be present. Even these least fermentable fluids of our third subclass invariably become turbid within three days after inoculation with living units, if maintained at a temperature of about 70° F.; whilst when other more changeable fluids are inoculated, putrefaction ensues with equal certainty, though with much greater rapidity.

* See 'Nature,' March 20th, 1873, p. 380.

What we have learned, therefore, concerning the *invariable uniformity* of simple inoculation experiments should of itself teach us how difficult it would be to account for cases of delayed putrefaction, or for cases in which a mere smouldering fermentation is set up, by the old though now well-nigh exploded notion of contamination by preexisting germs. Where living ferments really exist, the course of events is definite and almost *invariable* in its rapidity; but where fermentation takes place as a result of chemical changes occurring in the fluid itself (either by its own unaided powers, or under the stimulating influence of a less-heated organic ferment) there is abundant room for all the irregularity and variation actually encountered. These cases of irregularity and variation have always, on other grounds, defied all legitimate attempts to bring them individually within the pale of a narrow and exclusively "vital" theory of fermentation; and now a wider experience with living ferments equally tends to show the impossibility of legitimately explaining a great mass of irregular phenomena by means of agents whose action is shown to be constant and almost *invariable*.

Thus it can now be proved, by evidence of a most unmistakable nature, that the process of putrefaction which invariably occurs in previously boiled putrescible infusions contained in flasks with narrow but open necks is not commonly (is, perhaps, only very rarely) initiated by living germs or organisms derived from the atmosphere; it can also be proved that putrefaction and the appearance of swarms of living organisms may occur in some boiled fluids when they are simply exposed to air which has been filtered through a firm plug of cotton-wool or through the narrow and bent neck of a flask, to air whose particles have been destroyed by heat, or even in fluids hermetically sealed in flasks from which all air has been expelled. The evidence in our possession is therefore most complete on this part of the subject: it shows beyond all doubt, not only that putrefaction may and does very frequently occur under conditions in which the advent of atmospheric particles, whether living or dead, is no longer possible, but also that living particles derived from the atmosphere can only be very rare and altogether exceptional initiators of the putrefaction which invariably occurs in previously boiled infusions exposed to the air.

Again, the evidence which we now possess with reference to the influence of heat upon *Bacteria*, *Vibriones*, and their supposed germs is no less decisive. It has been unmistakably proved that such organisms and their imaginary germs are either actually or potentially killed by a brief exposure to the temperature of 140° F. when in the moist state; and it had also been previously established that they are invariably killed by desiccation even at much lower temperatures*.

* See the experiments and conclusions of Dr. Burdon Sanderson in Thirteenth Report of Med. Officer of Privy Council, p. 61. This fact of the inability of these or-

But if living germs do not come from the air to contaminate the previously boiled fluids, and if it is not possible for any of them to have escaped the destructive influence of heat in the boiling fluid or on the walls of the vessel in which the fluid is contained, what can be the mode of origin of the swarms of living things which so rapidly and invariably appear in such infusions when contained in open flasks, and which so frequently appear when the infusions are contained in flasks whose necks are closed against atmospheric particles of all kinds? They can only have arisen by the process which I have termed Archebiosis.

Conclusions.

If a previously boiled ammoniac-tartrate solution remains free from *Bacteria* and *Vibriones* when exposed to the air, it is because the air does not contain living organisms of this kind or their supposed germs, and because mere dead organic particles are not capable of initiating putrefaction in such a fluid.

And if ordinary organic infusions previously boiled and exposed to the air do rapidly putrefy, though *some* of the same infusions when exposed only to filtered air remain pure, it is because such fluids are, in the absence of living units, quite amenable to the influence of the dead organic particles which the air so abundantly contains, although they are not self-fermentable.

Whilst if other more changeable fluids, after previous boiling, when exposed to filtered air or cut off altogether from contact with air, do nevertheless undergo putrefaction or fermentation, it is because these fluids are self-fermentable, and need neither living units nor dead organic particles to initiate those putrefactive or fermentative changes which lead to the evolution of living organisms.

ganisms and their germs to resist desiccation shows the futility of some objections which have been from time to time raised by those who thought that *Bacteria*, *Vibriones*, and their germs might resist the destructive influence of heat by adhesion to the glass above the level of the fluid, or even in the fluid itself, just as dried and very thick-coated seeds have been known to do. Dry heat would seem to be even more fatal to such organisms and their germs than a moist heat of the same degree, owing to their extreme inability to resist desiccation: if they become dry they are killed at a temperature of about 104° F., whilst if they remain moist they succumb, as we have seen, to a temperature of 140° F.

May 29, 1873.

Sir GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

The CROONIAN LECTURE, "On Muscular Irritability after Systemic Death," was delivered by BENJAMIN WARD RICHARDSON, M.D., F.R.S.

(Abstract.)

The Lecturer commenced by referring to the labours of Dr. Croone, the Physician with whose name this lecture is connected. Croone was distinguished as one of the founders and as first Registrar of the Royal Society, and his scientific work included important contributions to the subject of muscular motion. Immediately preceding Croone, Nicholas Stenon, of Copenhagen (first an anatomist and afterwards Bishop of Heliopolis, a man whom Haller has described as *vir industrius, candidus, innocuus et magnus inventor*, and whom the Grand Duke of Tuscany buried in the tomb of his Royal house), had made known the fact that the contractile portion of muscle is resident in the carneous or fleshy part of the muscle, as distinguished from the fibrous part. This discovery, seized by Croone, led him to observe that the fleshy part of muscle is made up of fibres, and that each fibre possesses a distinct power of contraction; so that, to use his own words, "the force of the whole muscle is but an aggregation of each particular fibre."

Upon this observation, perfectly original at the time it was announced, Croone advanced an hypothesis as to the cause of muscular motion. He showed that for muscular contraction it was necessary that the arteries should supply blood to the fleshy muscular fibres—that the blood should pervade the fibres—that in its course, forced on by the stroke of the heart, it should mix with another liquor within the muscles, and diffuse into the minute vesicles of which each muscular fibre is ultimately composed. To complete the mechanism for motion, he held that the nervous filaments which ramify into each muscle supply a refined fluid, much more active than the muscular fluid, by which the activity of the muscle is called into play.

The lecturer next briefly traced the various hypotheses that have been advanced to explain muscular motion since the time when Croone made his original observations, the purport of the argument being that, although great advances have been made in the study of structure, and one very great advance in the study of function, viz. by Haller in his separation of the respective forces of nerve and muscle, the precise question of the cause of muscular motion remains much in the same doubt as Croone left it.

Respecting the various theories and hypotheses since the time of

Croone, the author had nothing to say in detail; the object of his lecture was to put forward certain facts he had learned on the phenomenon of muscular irritability after systemic death—that muscular irritability which remains sometimes for a brief, at other times for a long period after all the outward manifestations of life have ceased, and when, to the common apprehension, the animal tissues are dead. He included in the same study certain examples in which muscular irritability has for a time ceased, but has become redeveloped under new conditions. He thus included the study of those states which favour the continuance of irritability or which destroy it, and those conditions which suspend it but do not destroy it. By this method of research the author thinks we may proceed backwards towards living irritability, and may determine upon what that depends with more facility than by experimenting on the phenomena of irritability in the living animal. He imagines that if he knew nothing of the construction of a watch, or why for a certain time a watch maintains its motion, and if he had nobody to teach him these things, he might be better able to arrive at the fact he wanted by trying to set the motionless watch into motion than by interfering with it while it is in motion.

The record of experimental endeavour carried out with the design above explained included a review of the work of twenty-five years. The subjects brought under consideration were arranged as follows:—

- (1) The effect of cold on muscular irritability after systemic death.
- (2) The effect of motor forces, mechanical, calorific, electrical.
- (3) The effect of abstracting and supplying blood.
- (4) The effect of certain chemical agents, inorganic and organic.

Effect of Cold.

Previous to the time of John Hunter it was supposed that cold was the most effective agent for destroying muscular irritability, and to this day the impression is commonly maintained; so that the sensation of cold in a motionless animal is accepted as the surest evidence of death. Hunter was the first to show by direct experiment that this was an error, and that cold suspends irritability without destroying it. The original experiment of Hunter illustrative of this position was here described. The effects of cold employed in various ways in the author's experimental researches were now detailed systematically. The effect of cold in suspending the muscular irritability of fish, reptiles, and frogs was first described. On all these animals it was shown that cold could be made to suspend without destroying the muscular irritability for a long period of time, and that in fish (carp, on which the author had made the greatest number of experiments) the restoration of irritability could be perfected to the extent of the restoration of the living function.

Passing to warm-blooded animals, the author showed that in the process of cooling in every animal that has been suddenly deprived of life with-

out mechanical injury there is a period in the process of cooling when general muscular irritability may be made manifest. He demonstrates this fact by the simple experiment of throwing a current of water heated to 115° Fahr. over the arterial system of the recently dead animal. If the surrounding temperature be high at the time of this experiment, the operation should be performed within a few minutes after death; but if the temperature be below freezing-point, it may be delayed for a long period. In one experiment the author reproduced active muscular contraction in an animal that had lain dead and exposed to cold 6 degrees below freezing-point for a period of three hours. In this case the muscles generally remained irritable for seven minutes after the injection of the heated water, while in the muscles of the limbs, by repeating the injection at intervals, the irritability was maintained for two hours.

The author drew a comparison between these experimental results and the phenomena of muscular irritability that have been observed in the human subject after death by cholera. A short description of the muscular movements occurring sometimes after death from cholera was introduced. The movements were not conscious, nor were they promoted by electrical excitation; but the flexors and extensors belonging to each part in which there is movement are alternately contracted and relaxed as if from some internal influence. The same observations apply to the phenomena of contraction and relaxation in the muscles of animals that have been held in abeyance by cold and have been recalled into action by the injection of heated fluids.

The influence of cold in suspending without destroying muscular irritability was further evidenced by the experiment of subjecting some young animals to death, by the process of drowning them in ice-cold water. It was shown that in the kitten the muscular irritability may be restored to the complete reestablishment of life after a period of two hours of apparent systemic death, although the muscles when the animal is first removed from the water may give no response to the galvanic current. This same continuance of irritability after apparent systemic death by drowning in ice-cold water has been observed in the human subject, not in so determinate, but in an approximated degree. An instance was adduced in which a youth who had been deeply immersed for twelve minutes in ice-cold water retained muscular irritability so perfectly that he recovered, regained consciousness, and lived for a period of seven hours.

Commenting on the method of irritability, the author showed that a certain period of time is required before the irritability is raised from a mere passive condition, in which it responds only to external stimuli, into the condition necessary for independent active contractility. The change of condition from the passive to the active state, when it occurs, is so sudden as to seem instantaneous at first, then it is slowly repeated. This rule holds good in respect to voluntary muscles and involuntary. It is specially true in regard to the heart, which organ, the author states,

may perform its office under two distinct degrees of tension or pressure—a low tension, in which the organ itself is reduced in size and moves almost insensibly; and a full tension, in which it is of larger size and moves with a sufficient power to impel the blood so as to overcome the arterial elasticity and the capillary resistance.

Another fact bearing on this subject is, that in rapid decline of muscular irritability the muscles most concerned in the support of the organic functions, namely the heart and the muscles of respiration, are the last to yield up their spontaneous power; but when they have lost their power, they are the last to regain it. To this rule there is one exception, viz. in the muscular fibre of the right auricle of the heart.

The author then explained that the degree of cold which suspends irritability is fixed within certain measures of degree, from 38° to 28° F. being the most favourable degrees of cold. Above a temperature of 38° Fahr. the muscles in a little time pass into permanent rigidity, *rigor mortis*. Below 28° the muscles, if the effect of the cold be extended to their whole structure, pass into some new molecular condition from which they do not return into active life, at least they do not by means of any process of recovery of which we are at present conversant.

Effect of Motor Forces.

Cold, by the inertia it induces, suspends, under certain conditions, but does not destroy, muscular irritability. The motor forces, on the contrary, quicken the irritability for a brief period, and then completely destroy it. A method may perhaps be discovered for overcoming the effects of these forces, but at present it is not known. The mode in which all the motor forces act in arresting irritability is by the induction of a contractile state, which, once established, remains permanent. It should be remarked, however, that the forces respectively named mechanical, calorific, electrical, act with different degrees of intensity, perhaps because we cannot as yet apply them, in this particular research, with equal measures of intensity. The author here related his experiments on the effect of the different forces upon the right auricle of the heart, and reported as the result of his observations that, while all the forces act ultimately alike in producing permanent contraction, the mechanical excitation is much slower than the calorific; while electrical excitation appears to hold an intermediate place, as if it were a combination of mere mechanical motion with an increased temperature. Electrical tension may nevertheless be increased so as to rival heat in its immediate effect on contraction.

The author here traced out the results of a series of short sharp irritations of muscle with a needle-point, and compared them with the effect of a blow, showing that in each case rigidity follows, but is much slower in development when it is excited by the needle.

The influence of heat in destroying irritability, by its power of pro-

ducing permanent contraction, was described from experiments bearing on the relation of temperature to the muscular contraction of different animals—frogs, pigeons, and rabbits. It was shown that a relative rise in temperature in each class, a rise averaging 12 degrees in Fahr. scale, from the natural temperature of the animal is efficient for producing permanent rigidity, the cause of the ultimate rigidity being coagulation of the coagulable muscular fluid.

The effect of electrical excitation is in the same direction, but is varied according to the mode in which the excitation is performed. Discharge from the Leyden jar produces contraction, which is permanent or intermittent in accordance with the mass of the muscle and the intensity of the discharge. This fact was elucidated by reference to a series of experiments with a Leyden battery, placed in cascade, and the effect produced by the discharge from 96 feet of surface upon animals of different sizes and weights, from sheep down to pigeons, as well as on sections of the bodies of the same animals immediately after death. The experimental facts demonstrated that with an efficient discharge the whole muscular system of a small animal could be fixed instantly in the rigidity of death, and that the precise position of the animal at the period immediately preceding death was retained with such perfection, so sudden was the change, that nothing but physical examination by the hand could bring to the mind the fact that the animal had passed from life into death.

But the same shock passed through a sheep weighing 54 pounds produced only a temporary contraction of muscle, and required several repetitions before the rigidity was rendered permanent.

By employing discharges of lower tension it was found that muscles, or special tracts of muscles, in the same animal, immediately after its death, could be made rigid quickly or slowly by variation of the intensity of the discharge.

The effect of the intermittent electro-magnetic current was next brought forward, and was shown to resemble closely that of the simple electrical discharge from the Leyden phial. Intensified it induces instant and permanent contraction; and if it be repeated, even with but sufficient force as to call forth feeble contraction, it destroys the irritability, *ceteris paribus*, more quickly than if the muscle had been left at rest.

Parenthetically, the lecturer dwelt here on the common practice after sudden death of endeavouring to excite the action of the enfeebled heart by passing through it an electrical current. Some practitioners, said the author, have gone so far as to introduce a needle into the heart itself, and to make the needle act as one of the conductors from a battery. Such experimentalists, before they undertake this operation on the human subject, should at least observe the effect of the agency they are employing on the exposed heart of an inferior animal recently and suddenly killed by drowning or by a narcotic vapour. They would learn

then with what infinite facility the muscular irritability of the heart, in all its parts, is excited, for a moment only, to be permanently destroyed. They would learn that if blood be not passing through the muscular structure concurrently with the exciting agent, they could not more effectually arrest function than by the very method they have adopted to sustain it.

The influence of the continuous current on muscular irritability was introduced by the author, together with a special reference to the first experiments of Aldini on the bodies of malefactors who had been recently executed; and it was shown from Aldini's most noted experiment how largely the phenomena of motion he induced in a dead man, and the recital of which caused so much sensation in the year 1803, was due, not to the galvanism, but to the circumstance that the dead body had been exposed for the hour after death, and before the experiments commenced, to the action of cold two degrees below freezing-point. On the whole the continuous current acts on muscular fibre after the manner of heat. If the muscle, recently dead, be exposed to cold, the current, when sufficient, restores for a limited period the irritability, and finally destroys it by inducing persistent contraction; if the muscle, recently dead, be left at its natural temperature, the current simply shortens the period of irritability by quickening contraction.

Abstraction and Supply of Blood.

Under this head the author first considered the effect of abstraction of blood from the living muscular fibre. He showed that when the flow of blood was very rapid, there was invariably a given period of muscular excitation. In sheep killed in the slaughterhouse he found that this muscular excitement occurred at the time when the proportion of blood removed from the animal was equivalent to about the 320th part of the weight of the animal. The increased irritability passes rapidly into general convulsion without consciousness, and, as a rule, ceases for a time with a temporary cessation of further loss of blood. After this the irritability remains, if the bleeding be arrested altogether, and can be called into action by any external stimulus, although it is rarely spontaneously manifested when the vessels are left divided and open. After an interval of one or two minutes there is a recurrence of loss of blood, followed by a muscular excitement which marks the moment of systemic death. The breathing and circulation cease, but the voluntary muscles retain their irritability for several minutes, until they undergo permanent contraction; indeed they retain their irritability under the influence of cold and lose it under the influence of mechanical motion, heat, or electricity as markedly as when death has been produced without abstraction of blood.

The fact of the two stages of exalted muscular irritability during abstraction of blood is important, as indicating the two different tensions

of muscle to which reference has already been made. The first convulsive action, convulsion of syncope, marks a definite period, when the tension of the heart and therewith of the whole vascular system is reduced to a degree of action well defined and attended with definite phenomena. In this stage the force of the heart is sufficient to move the unconscious muscles, and gradually, if blood be resupplied, to lift them again into the condition for the sudden development of active conscious life. The second excitement, convulsion of death, indicates the period when the passive or lower tension of the muscular power ceases.

A distinction was here drawn by the author between the muscular condition present during syncope and during death. Syncope, it was urged, means the continued action of the heart at a low tension, from which it can be suddenly raised into full tension with restoration of the powers of life ; death means the cessation of the lowest tension at which the heart can effectively work.

It was shown that in all the cases of restored animation after apparent death the condition of the heart was that of a muscle acting under the lower degree of tension. In this intermediate stage, between syncope and death, the most striking results were obtainable ; but beyond this stage the methods so successful during it, were practically useless for restoration.

The experiments of the author for reestablishing artificial respiration, together with artificial circulation, and of these combined with electrical excitation of the nervous centres, were next referred to ; but as they had already formed the subject of a paper read before the Society, they were but briefly dwelt upon. If, continued the author, the question be asked, why, at a certain stage of hæmorrhage, there is development of muscular excitability, the answer is not difficult. The phenomenon is probably due to an irregularity in the distribution of power between the muscular organ and the nervous centre with which it is connected : the effect is due, that is to say, to a continued nervous irritation applied to the muscle after the resistance of the muscle is impaired, while the cessation of the motion is due to the nervous exhaustion that succeeds. In this first series of changes the voluntary muscles and the voluntary centres are involved ; but the loss of blood continuing, the same series of changes affect in turn and in like order the involuntary muscles and their nervous centres : the result of this is the second convulsion, indicating death, after which there is no further motion except that which belongs to particular parts ; there is no central motion, that is to say, supplying all parts as from a prime source of power, and sustaining all parts from a prime direction of the motion supplied.

The author described in this place a number of experimental attempts to preserve blood for the purposes of transfusion. He had endeavoured to preserve by slow desiccation of blood ; but had found that when blood was submitted to evaporation, however slowly, an albuminous pellicle formed on the upper surface, exactly as caseine forms on milk, and this

prevented the perfection of the process. Recently he had been more successful by evaporating blood to which saline substances had been added; he could not, at the same time, pronounce what he considered a satisfactory termination of this line of experimental inquiry.

Effect of some Chemical Agents.

In this portion of his lecture the author detailed a series of experimental researches with various chemical substances, organic, inorganic, and intermediate, which tend to prolong the period of muscular irritability by diffusion through the tissues of animals recently dead. The substances which suspend irritability act in two ways. Some, like chloride of sodium and other soluble saline substances, act merely by holding the coagulable fluid of the muscular tissue in a continued state of fluidity; others seem to have a different action, and to hold the nervous function also in suspense. The nitrite of amyl and other members of the nitrite series belong to this last-named class of agents, and some of the cyanogen bodies exert a similar influence. In experiments with nitrite of amyl on cold-blooded animals (frogs), the author had suspended muscular irritability for a period of nine days, and had then seen it restored to the extent even of restoration of life. In one instance this restoration took place after the commencement of decomposition in the web of the foot of the animal. In warm-blooded animals a series of suspensions had been effected by nitrites and also by cyanogens, not for so long a period, but for periods of hours, in one instance extending to ten hours.

Under the head *Action on Nervous Matter* the author included, finally, a description of certain experiments he had made to determine the direct effects of some agents upon the nervous matter. In the whole series of his inquiries no fact had impressed him more forcibly than this, that the muscular irritability, in so far as it belongs to the muscle, may be sustained for hours after the nervous excitation which calls it into spontaneous action has ceased. Hereupon he infers that after death the nervous matter undergoes a change of condition which, in result, is identical with that change in muscle which we call *rigor mortis*. There is evidence, moreover, from some rare cases, that the final inertia of nervous matter may be suspended and revived, so that all the muscles may be reanimated. This point was elucidated by reference to the phenomena that had recently been observed by Mr. Wadswale Watson, of Newport, Monmouthshire, on a double monster, photographs of which were placed before the Society. In this instance two children were born so attached that the separation of them was impossible. Both lived equally for three hours after birth, and then one died and remained dead for three hours, while the other lived. At the end of the time named the dead child recommenced to breathe, and showed other signs of restored muscular power; then it sank into a second death, but at intervals of about four hours moved again; at length, twenty-three hours after its first apparent death, during a fit of

crying of the living child, it recovered sufficient power to breathe and even to cry, and manifested evidence of life in all its muscles, except the heart, for twenty minutes, when it had a severe convulsion, which stopped all further motion.

In this instance the author believed that the retention of muscular irritability had depended upon the retention in the nervous organism of the conditions necessary for exciting motion. He proceeded to describe his researches as to the possibility of suspending nervous changes incident to death, so as to retain the conditions requisite for the communication of nervous impulse to muscular fibre.

SUMMARY.

1. There are three degrees of muscular irritability—the active efficient, passive efficient, negative inefficient. The muscle after death may be suspended in any of these conditions for action; but, as a rule, it is the last condition only that is maintained long after death.

2. Muscular irritability may be suspended or stopped altogether under three different conditions, having reference to its connexion with nervous activity:—(a) The nervous and muscular activities may be suspended equally, on which there may follow spontaneous return of motion. (b) The muscular irritability may outlive the nervous function, on which the phenomenon of irritability may be induced by the application of the motor forces, but there is no return of spontaneous irritability, *i. e.* of irritability belonging to the animal as an independent agent. (c) The nervous function may outlive the muscular irritability, under which circumstances irritability is invariably stopped by the production of persistent contractility of the muscular fibre.

3. Nervous activity exciting muscular action is identical with all the motor forces, and particular to none. It is equivalent to mechanical, calorific, or electrical force, and equally susceptible of manifestation through either.

4. Muscular irritability after death is possible under conditions which, so far, are thus known. Cold, in certain defined degrees, suspends without destroying it. The motor forces strike it into rest. Blood sustains it or stops it according to the balance of power existing between the muscular and nervous systems. Some chemical agents suspend it independently, others suspend it, together and equally, with suspension of the nervous function. When suspension is equal there may be spontaneous return; when it is unequal there is no return.

In conclusion, the author said there had been six great advances in physiological medicine—*viz.* the introduction of the art of anatomy, the discovery of the circulation of the blood, the discovery of the chemical process of respiration and nutrition, the discovery of the ultimate structure of animal bodies, the discovery of osmosis and of the crystal-

loidal and colloidal conditions of animal matter, and the discovery of methods for the general and local extinction of pain. The next grand advance has for its design the discovery of the precise relationships of nervous to muscular structure in the living and in the dead conditions. That established, the phenomena of muscular movement, its suspension or its destruction, will be understood like every other physical fact that lies this side of ultimate fact—that is to say, that lies, as this clearly does, within the range of experimental inquiry.

June 12, 1873.

The Annual Meeting for the election of Fellows was held this day.

SIR GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Lieut.-Col. Strange and Dr. Webster were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society:—

William Aitken, M.D.	Lieut.-Col. James Augustus Grant,
Sir Alexander Armstrong, M.D.,	C.B., C.S.I.
K.C.B.	Clements Robert Markham, C.B.
Robert Stawell Ball, LL.D.	George Edward Paget, M.D.
John Beddoe, M.D.	George West Royston-Pigott, M.D.
Frederick Joseph Bramwell, C.E.	Osbert Salvin, M.A.
Captain Edward Killwick Calver,	The Hon. John William Strutt, M.A.
R.N.	Henry Woodward, F.G.S.
Robert Lewis John Ellery, F.R.A.S.	James Young, F.C.S.

Thanks were given to the Scrutators.

June 19, 1873.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

Sir Alexander Armstrong, Dr. Robert Stawell Ball, Mr. Frederick Joseph Bramwell, Captain Edward Killwick Calver, Lieut.-Col. J. Augustus Grant, Mr. Clements Robert Markham, Dr. George West Royston-Pigott, Mr. Henry Woodward, and Mr. James Young were admitted into the Society.

The following communications were read:—

- I. "Experiments on the Development of *Bacteria* in Organic Infusions." By C. C. POPE, M.B., Demonstrator to the Regius Professor of Medicine, and E. RAY LANKESTER, M.A., Fellow and Lecturer of Exeter College. Communicated by Dr. H. W. D. ACLAND, Radcliffe Librarian and Regius Professor of Medicine in the University of Oxford. Received April 16, 1873.

The following passage from Dr. Charlton Bastian's 'Beginnings of Life' (vol. i. p. 429) induced us to make experiments similar to those mentioned in it, with the view of testing the correctness of Dr. Bastian's conclusion as to matter of fact:—

"On the other hand, the labours of very many experimenters have now placed it beyond all question of doubt or cavil that living *Bacteria*, *Torula*, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F., even for one or two hours. This result is now so easily and surely obtainable, as to make it come within the domain of natural law." And in a note is added, "In a very large number of trials I have never had a single failure when an infusion of turnip has been employed; and from what I have more recently seen of the effects produced by the addition of a very minute fragment of cheese to such an infusion (see Appendix C, pp. xxxiv-xxxviii), I fully believe that in 999 cases out of 1000, if not in every case, a positive result could be obtained." Though this is one out of a great number of statements made by Dr. Bastian upon which he bases speculations as to the prevalence of spontaneous generation or archebiosis, we think it necessary to state that we have not considered that (which is a question of interpretation) as the point at issue, but merely the question of fact as to the appearance of *Bacteria* in what may be considered, according to our present lights, infusions duly guarded from inoculation. The point under discussion is one as to a fact in the natural history of *Bacteria*, in a further study of which we are occupied at the instance of the Radcliffe Trustees; and we believe that a more precise knowledge of the life-history, life-conditions, and various forms of these organisms is necessary before the hypothesis of their spontaneous generation can serve as a safe guide in scientific investigation.

The experiments recorded below were made with infusion of hay and with infusion of turnip, sometimes with the addition of a few fragments of pounded cheese. It is necessary at once to call attention to three precautions which we have taken, and which we think are indispensable:—

1. Recognizing the fact that the presence of lumps is a possible source of error, we excluded these from our infusion, either by filtration or by decantation.
2. To ensure the satisfactory exposure of the whole contents

of the tube to the boiling temperature, we, as a rule, *completely* submerged our experimental tubes in boiling water for a period varying from five minutes to half an hour. 3. The substances used in preparing the infusions being necessarily of a very heterogeneous nature, we always examined samples of the infusions before and after boiling, at the time of closing the tubes, and were thus able to determine whether any *change* had taken place in the visible particles contained in the fluid after a lapse of time.

The microscopes used by us throughout, working side by side with samples from the same infusion, were a Hartnack's Stative VIII. objective No 10 à immersion, ocular 4, belonging to the anatomical department of the University Museum, and a large Powell and Lealand belonging to the Radcliffe Trustees, which is provided with a $\frac{1}{2}$ and a $\frac{1}{6}$ objective. The former of the two English glasses was more usually employed than the latter, on account of its greater convenience in manipulation.

Appearances in freshly prepared infusions.—Since the objects seen in such infusions are remarkable, and have doubtless sometimes led to error in subsequent examination of infusions, we may draw attention to them now. In such freshly prepared infusions we have not unfrequently seen appearances agreeing very closely with some of those figured by Dr. Bastian in his book as coming into existence *after* boiling, sealing, and preservation in a warm chamber. A freshly prepared and boiled strong infusion of hay may present shreds of vegetable fibre, a considerable number of dead *Bacterium termo* (some two or three to the field), minute, highly refringent spherules, varying from the size of a blood-corpuscle to the smallest size visible; and such spherules are often present in pairs, forming figure-of-8-shaped bodies, both smaller and larger than *Bacterium* and of different optical character. Further, dumb-bell-shaped bodies are not unfrequently to be observed of similar form and size to *Bacteria*, but coarser in outline; they dissolve on addition of HCl, which *Bacteria* do not*. All these bodies exhibit constant oscillatory (Brownian) movements. The addition of new cheese to such an infusion (as shown by examination of a simple infusion of new cheese taken by itself) adds a considerable number of highly refringent spherules of various sizes (oil-globules) and finely granular flakes, also a few *Bacteria* and (if the cheese be not quite new, almost certainly) fungus-mycelium and conidia in quantity.

Fresh-boiled turnip-infusion alone may contain so very few dead *Bacteria* that none are detected with the microscope, or only one in a drop. It presents a great number of minute, highly refringent spherules, varying in size from $\frac{1}{5000}$ inch downwards, all in most active oscillatory

* In the most carefully guarded of the experiments published by Dr. Child a few years since in the 'Proceedings of the Royal Society,' a very small number of bodies similar to these were obtained; and we suggest that they were of the same nature.

movement. Shreds and filaments of various sizes and character also are found, and a few finely granular flakes about $\frac{1}{1000}$ inch in diameter. The addition of cheese brings in, of course, the objects enumerated above as belonging to it.

Visibility of Bacteria.—It is perhaps necessary to say, before proceeding further, that we have satisfied ourselves that, in infusions of the optical character of those used, the multiplication of *Bacteria* makes itself obvious by a cloudiness. Hence, though we have not remained content with that evidence, the retention by such a limpid infusion of its limpidity is a proof of the absence of *Bacteria*. We also should mention, what is well known already, that in a closed tube or bottle, after such a cloud (of *Bacteria*) has developed, the *Bacteria* at a certain period cease to multiply and settle down as a fine powder, leaving the fluid again clear. Such precipitated *Bacteria* remain unchanged in the fluid for a long period (weeks certainly, perhaps years), and can be readily shaken up and at once recognized by microscopic examination; they are, moreover, not destroyed by boiling: hence it is not possible to miss the detection of a development of *Bacteria* in a limpid turnip-infusion, examined daily for three weeks or more by the naked eye, and finally, after agitation, by means of the microscope.

SERIES A. Nov. 23rd. *Experiments with hay-infusion.*—An infusion was prepared by pouring water of about 90° C. on to chopped hay. The infusion was of a dark sherry-colour; reaction slightly acid. The glass tubes used in this and subsequent experiments were about five inches in length, of half inch bore, rounded at one end and drawn out to a capillary orifice at the other. The infusion in these and subsequent experiments was introduced by heating the tube and plunging its capillary beak beneath the surface of the experimental liquid during the cooling of the expanded air, until the tube was about one third or half filled. Tubes 1, 2, 3 were half filled with the hay-infusion previously filtered, the liquid was boiled in the tube, and the capillary beak fused, as nearly as possible, during ebullition*.

Tubes 4, 5 were similarly treated, with the difference that a small quantity of cheese, in a very fine state of division, had been added to this portion of the hay-infusion before its introduction into the tubes.

Tubes 6, 7. Quantity and character of the infusion as in 1, 2, 3, but the tubes sealed without previous ebullition.

* The tubes were sealed at the moment of removal from the flame over which they had been boiling. In every case a subsequent recurrence of ebullition was observed during the cooling of the upper part of the tube. Dr. Roberts, of Manchester, has suggested that the occurrence of *Bacteria* in tubes thus sealed may be explained by their in-draught, together with a certain amount of air, at the moment of closure; but the experiments of Sanderson, recently confirmed by Cohn, have shown that contamination of fluids by *Bacteria* only takes place through the medium of impure surfaces or liquids.

Tube 8. Quantity and character of the infusion as in 4 and 5, but the tube sealed without previous ebullition.

Tubes 9, 10, 11. Quantity and character of the infusion as in 1, 2, 3, but rendered slightly alkaline with KHO. Sealed approximately during ebullition.

All these tubes (1 to 11) were after closure completely submerged in boiling water for fifteen minutes, and were then preserved in a hot-air bath, varying in temperature from 30° C. to 35° C.

Microscopic and naked-eye appearances of the hay-infusion at the time of sealing the tubes.—The infusion in tubes 1, 2, 3, 6, 7 was clear and pellucid, that in tubes 4, 5, 8, 9, 10, 11 was hazy.

Microscopic examination gave the result indicated above, as to the appearances of freshly prepared hay and hay-and-cheese infusion.

Subsequent appearances of the infusion in Tubes 1-11.—The tubes with infusion which was pellucid at the first were found to retain this character for several weeks, being preserved in the air-bath and examined from day to day. The hazy infusions were opened after four days, and their contents found to be unchanged.

A portion of the same hay-and-cheese infusion, boiled and purposely contaminated by preservation in an uncleaned beaker, was found after four days to be teeming with *Bacterium termo* exhibiting vital movements. The pellucid infusions were subsequently examined with the microscope at different times and found to be unchanged.

SERIES B. Nov. 25th. *Experiments with turnip-and-cheese infusion.*—An infusion was made with 700 grms. sliced white turnip and 1000 grms. water, to which about 1 grm. finely minced new cheese was added, the jug containing the mixture being maintained for four hours on a sand-bath at a temperature of 45°-55° C.

The infusion was now filtered; sp. gr. of the infusion 1011.1. Reaction slightly acid.

Tubes 12, 13, 14. Sealed cold. Submerged in boiling water for thirty minutes.

Tubes 15, 16, 17, 18, 19. Sealed approximately during ebullition. Submerged in boiling water for thirty minutes.

The tubes were preserved in the air-bath as in Series A.

Microscopic and naked-eye appearances of the infusion at the time of sealing the tubes.—The liquid in all the tubes was perfectly clear and limpid. A few shreds and flakes were obvious, which appeared to be derived from the filter-paper and from the slight precipitation of albuminous matter. The microscopic appearances were those above described as characterizing such infusions.

Subsequent appearances of the infusion.—The infusion in all the tubes was found on examination from day to day to retain its limpidity. Subsequent microscopic examination of all the tubes at various periods subsequent to the closure of the tubes (from four days to three weeks)

yielded no indication whatever of a development of *Bacteria* or other organisms, nor of any change. A portion of the same infusion placed in an uncleaned beaker for comparison was milky and swarming with *Bacteria* after three days.

SERIES C. Nov. 28th. *Experiments with turnip-and-cheese infusion.*—The infusion similar in all respects to that in series B, but prepared with a somewhat larger proportion of turnips; therefore of higher specific gravity, which was not numerically determined.

Tubes 20, 21, 22, 23. Boiled and sealed approximately during ebullition. Not subsequently submerged.

Tubes 24, 25. Boiled and sealed approximately during ebullition. Subsequently submerged in boiling water during thirty minutes.

The tubes were preserved in the air-bath as in Series A and B.

SERIES D. Nov. 30th.—An infusion prepared as in Series B and C, but brought to a sp. gr. 1031 by evaporation after filtration.

Tubes 26, 27, 28, 29. Sealed cold. Subsequently submerged in boiling water for thirty minutes.

Tubes 30, 31. Boiled and sealed approximately during ebullition. Not subsequently immersed.

Tubes 32, 33. Boiled and sealed approximately during ebullition. Subsequently submerged in boiling water for thirty minutes.

Appearances in the infusions, Series C and D, at the time of sealing and submerging.—The appearances in the freshly prepared infusion were similar to those described above as characterizing such infusions.

Subsequent naked-eye examination of the tubes did not reveal the slightest change; they remained limpid. Specimens from each group were opened and examined with the microscope after four days, and the microscopic characters found to be unchanged: the liquid was perfectly sweet. The remaining tubes were examined at intervals before the end of December, being maintained during the whole time at a temperature of 35° to 40° C. in the air-bath; they equally proved to have remained unchanged when opened and examined with the microscope, and were also free from unpleasant smell.

SERIES E. Nov. 28th.—Six porcelain capsules were heated to redness, and nearly filled with the turnip-infusion used in Series C. They were placed on the air-bath under a glass shade.

Capsules 1, 2. The infusion was unboiled.

Capsule 3. The infusion was boiled in the capsule.

Capsule 4. The infusion was introduced after it had been boiled for five minutes in a superheated test-tube.

Capsules 5 and 6. The infusion was that used in capsule 4, but a drop of distilled water was added to each of these two capsules.

After four days the infusion in capsules 1, 2, 5, and 6 was found to be teeming with *Bacterium termo* and Bacterian filaments.

Capsule 3 was found to be cracked, and hence was discarded (it swarmed with *Bacteria*).

Capsule 4 was perfectly free from organisms, and remained so during a fortnight, when a fungus-mycelium made its appearance on the surface.

SERIES F. Dec. 10th.—A strong infusion of turnip and cheese, prepared as in Series B (sp. gr. 1013), was boiled in an eight-ounce flask for five minutes. Three common test-tubes were superheated and placed in a beaker to support them.

No. 1. The infusion was poured in, and with it one drop of distilled water.

No. 2. The infusion was poured in and thus left.

No. 3. The infusion was poured in and again boiled for two minutes.

These and the flask containing the remaining infusion were left on a shelf for one day; on Dec. 11, there being no cloudiness in any of the four, they were placed on the top of the hot-air bath. On Dec. 13 No. 1 was found to be swarming with *Leptothrix*-growths and free *Bacterium termo*.

No. 2 also was cloudy and swarmed with what Cohn calls the rosary-chains. No. 3 was absolutely free from all development of life, and was perfectly sweet and limpid; so also was the fluid in the original flask, a large one capable of holding eight ounces. How is the development of *Bacteria* in No. 2 to be explained? The original fluid remains pure; the fluid in No. 3, which was reboiled, remains so too; the tube itself, No. 2, had been heated red-hot and could not be a source of contamination. One's attention was therefore directed to the conditions of the passage of the fluid from the flask into the tubes; and here an explanation at once offered itself. The large flask *had not been superheated*; its lip was still dirty, laden with *Bacteria* ready to contaminate fluids as they poured from it; hence the contamination of the fluid in test-tube No. 2. The validity of this explanation cannot be disputed, because it is known that such glass surfaces, unless specially cleansed, invariably contaminate infusions exposed to them.

SERIES G. Feb. 11th.—The publication of Dr. Burdon Sanderson's letter, describing some experiments made by Dr. Bastian, induced us to make a further series of experiments with important modifications. We had expressly avoided the introduction of any thing like visible lumps of solid cheese or turnip into our infusions during their ebullition, believing that such lumps were a possible source of the exclusion of *Bacteria* or their germs from the killing influence of the boiling temperature. This precaution we had supposed (in the absence of any statement to the opposite effect) to have been taken by Dr. Bastian in the experiments adduced by him in the 'Beginnings of Life.' The presence of such lumps was publicly suggested in discussion at the British-Association Meeting at Liverpool as a source of fallacy, and has been demonstrated to be so by Dr. Ferdinand Cohn in experiments made with peas and infusion of peas ('Beiträge zur Biologie der Pflanzen,' Breslau, 1872). Further, we had limited the bulk of our infusions and the size of our experimental tubes, in view of the obvious consideration that the larger the mass and area to be guarded against contamination the greater the

chance of failure in that respect. Thirdly, it had not occurred to us to make use of vessels in these experiments of a form so inconvenient and difficult to thoroughly guard against effects of "spluttering," and to thoroughly heat by boiling, as the retort. Nor could we guess, in the absence of any directions on that point from Dr. Bastian, that it was desirable to exclude the rind of the turnip from the preparation of the infusion. The correspondence in 'Nature,' however, indicated that "pounded" cheese (necessarily in a condition of solid lumps) was added (in some cases) to his *experimental vessels after the turnip-infusion*, and was present during ebullition. It also appeared that retorts capable of holding two ounces were the vessels used; whilst, on grounds not given, it was considered advantageous by Dr. Bastian to peel the turnips before slicing them.

The following experiments were accordingly made:—

An infusion of turnip (minus the rind) was prepared and filtered; it had sp. gr. 1012·7. In the experiments Nos. 34 to 47 two-ounce retorts were used, and the bulb half filled with the experimental infusion.

No. 34. The infusion neutralized with KHO. About two grains of pounded cheese in pellets added to the retort.

Nos. 35, 36. Infusion not neutralized. About two grains of pounded cheese in pellets added to the retort.

Nos. 37, 38, 39. The simple infusion.

No. 40. The simple infusion, to which were added a few drops of an emulsion of cheese prepared with some of the turnip-infusion and new cheese, the emulsion having been filtered.

No. 41. The simple infusion.

Nos. 34 to 40 were boiled for five minutes; they were then preserved in the air-bath at a temperature of 35° C., and sealed approximately during ebullition. Four of them, including No. 36, were subjected to a further boiling of fifteen minutes in a water-bath after sealing.

No. 41 was boiled for five minutes and placed on a shelf with its mouth open.

Subsequent appearances in Retorts Nos. 34–41.

On Feb. 15th Nos. 34, 35, 37 were opened and found to be perfectly sweet and free from a development of *Bacteria* or other organisms.

No. 41 was observed to be perfectly limpid, and is so still (March 17th).

On Feb. 27th Nos. 36, 38, 39, and 40 were opened. With the exception of No. 36, they were perfectly sweet and free from organisms.

No. 36 had a slightly foetid odour and swarmed with rather long *Bacteria*—that is, *Bacteria* longer than the common *B. termo*, which develops in infusions open to atmospheric air, but not quite of the form of the *Bacillus subtilis* of the butyric fermentation, which is stated to appear in some infusions, *e. g.* milk, to which the access of atmospheric air has been entirely prevented. It is to be noticed that in this series the only retort in which *Bacteria* made their appearance was one of those in

which small lumps of cheese were present during the subjection of the flask to the process of ebullition and subsequent immersion in boiling water.

This result induced us to make a further series of differential experiments, bearing upon the influence of the state of aggregation of the cheese introduced into the turnip-infusion.

SERIES H. March 8th.—A turnip-infusion was prepared as in Series B; found after filtration to have sp. gr. 1113.5.

Tubes similar to those used in Series A–E, and half filled, were used.

Tubes 42, 43, 44. The simple infusion was poured into the tube, so as to half fill it; a lump of cheese the size of a pea was then added. Sealed cold.

Tubes 45, 46, 47. To the turnip-infusion, before introduction into the tubes, an emulsion of cheese prepared with turnip-infusion and strained through a piece of cambric was added. The tubes were then half filled with this mixture and sealed cold.

Tubes 48, 49, 50. The same as 42, 43, 44, but sealed approximately during ebullition.

Tubes 51, 52, 53. The same as 45, 46, 47, but sealed approximately during ebullition.

All the tubes, 42 to 53, were completely submerged during five minutes in boiling water, and subsequently preserved in the air-bath at 35° C. temperature.

On March 13th the contents of the twelve tubes were examined with the microscope. No. 45 had been broken in the boiling. The five remaining tubes which had been prepared with cheese in the finely divided condition were found to be entirely devoid of life, the infusion microscopically and otherwise unchanged. Of the six tubes prepared, each with a small lump of cheese, no organisms were detected in 42 and 44; but in 43 and 49 a few elongate *Bacteria* were observed (in the proportion of about two to the field of a Hartnack's system 10). In 48 and 50 the fluid was swarming with elongate *Bacteria* and true *Bacillus*. The lumps of cheese in those tubes in which life appeared had softened and spread out to a certain extent on the side of the tube. The cheese-lumps in Nos. 42 and 44 retained their original form.

From the result of these later experiments, made in consequence of the fuller information given by Dr. Sanderson as to Dr. Bastian's mode of treating turnip and cheese so as to obtain phenomena supposed to be in favour of the doctrine of Archebiosis, we consider that the importance of excluding visible lumps from the experimental infusions is clearly indicated, as also is the comparatively greater trustworthiness of the small tube as opposed to the larger retort for use as an experimental vessel. We moreover consider that we, in our earlier experiments (November and December), carefully following Dr. Bastian's directions, as far as he

had given any in the 'Beginnings of Life,' but using at the same time proper care as to cleanliness and due boiling, obtained a series of results contradicting Dr. Bastian's statements as to the spontaneous generation of *Bacteria* in infusion of turnip to which a fragment of cheese had been added.

Further, certain of the experiments above recorded, and others made at the same times with open vessels and simple turnip-infusion, compel us to dissent emphatically from the conclusion of the following statement contained in a recent paper by Dr. Bastian ('Nature,' Feb. 6th, p. 275):—"Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask, with the assurance that it contains no living organisms. Having ascertained also, by our previous experiments with the boiled saline fluids, that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion *invariably* becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*." The italics are our own.

We find not only that such an infusion remains free from *Bacteria* when thus treated (subject, of course, to certain failures in the precautions taken) for "one or two days," but if contamination by the admission of coarse atmospheric particles capable of carrying *Bacteria* be guarded against, it will remain so for weeks and probably so for years. In consequence of this absence of development of *Bacteria* we have cultivated *Torulae* in such a turnip-infusion, so as to obtain them entirely free from the former organisms*.

In conclusion, we would point out that failure in manipulation, contamination in unsuspected ways, such as that due to the preservative influence of lumps, and, again, the mistaking of particles in an infusion which have been there from the first for organisms originated *de novo*, do not exhaust the list of conceivable explanations of phenomena which have been attributed to spontaneous generation. When the knowledge of the natural history of *Bacteria* has advanced somewhat further, there will be a possibility of such explanations presenting themselves in ways at this moment unsuspected.

Whilst awaiting Professor Huizinga's fuller account of his experiments, we may point out that the hypothesis of an inhibitory influence of increased density should be supported by experimental evidence, and that it cannot apply to tubes closed before boiling. The neck of the flask closed with asphalt may (so far as conditions are stated by him at present) harbour *Bacteria*, as in our Series F. But especially we would urge upon him and others that it is undesirable, as yet, to introduce into

* At this moment, May 20th, the turnip-infusion in the open retort (No. 41) is free from all organisms, and is perfectly limpid and sweet.

the discussion other organic mixtures. Turnips and cheese may be very bad material for experiment; but it would be well, as far as possible, to settle the matter, or the way in which the matter is to be viewed with regard to them, before going off to other particular cases.

It would be a very excellent thing if all further reference to this subject could be postponed for a year or two—that is, until further study of *Bacteria*, such as that inaugurated by Sanderson and Cohn, has given us surer ground to tread upon.

II. "On the Nature and Physiological Action of the Poison of *Naja tripudians* and other Indian Venomous Snakes."—Part I. By T. LAUDER BRUNTON, M.D., Sc.D., M.R.C.P., and J. FAYRER, C.S.I., M.D., F.R.C.P. Lond., F.R.S.E., Surgeon-Major Bengal Army. Communicated by Prof. HUXLEY, Sec. R.S. Received April 22, 1873.

On the Poison of Naja tripudians.

The destruction of life in India by snake-bites is so great, that, with the hope of preventing or diminishing the mortality, in 1867 Dr. Fayrer began, and has recently completed, a protracted and systematic series of investigations on the subject in all its aspects; and, in a work entitled the 'Thanatophidia of India,' has published a description of the venomous snakes found in British India, with an account of a series of experiments on the lower animals, conducted for the purpose of studying the nature of the poison, its *modus operandi*, and the value of the numerous remedies that have been from time to time reputed as antidotes—that is, as having the power of neutralizing the lethal effects of the virus, and of saving life.

His object in carrying out these investigations has been:—

1st. To ascertain the nature and relative effects of the bite of the different forms of Indian venomous snakes, and the conditions and degrees of intensity under which the activity of the virus is most marked.

2nd. The physiological action of the virus, and its mode of causing death.

3rd. The value of remedies, and the extent to which we may, by preventive or therapeutic measures, hope to save life.

4th. To ascertain and make known the actual state of our information in connexion with these three points of inquiry, and to substitute scientific and rational knowledge for vague, empirical, and dangerous theories.

He has had the honour of submitting a copy of this work to the Royal Society; and it is therefore unnecessary to occupy its time by repeating much of what is therein related on the 1st, the 3rd, and part of the 4th heads.

But on that which is involved in the 2nd, and partly in the 4th, much is still required to be done; and therefore on the question of the nature and physiological action of the virus on life, and the application of that knowledge in the treatment of those poisoned, the following investigations have been made.

That the subject is one of interest in a purely scientific as well as sanitary point of view we believe will be admitted; for it is as important to humanity as to science that the nature and properties of a poison which, in India alone, probably destroys over 20,000 human beings annually should be determined.

We are aware that these figures may excite astonishment and even mistrust; but the sources from which the information is derived place it, we think, beyond a doubt, being derived from official returns for the year 1869, supplied to Dr. Fayer by the Government of India.

He has received reports from Bengal, the North-west Provinces, Punjab, Oude, Central Provinces, Central India, Rajpootana, British Burmah, showing the loss of life from snake-poisoning in those provinces in the year 1869.

These records represent, it is true, only a portion of India, as the Madras and Bombay Presidencies, as well as other parts of India, are not included. Had similar information been obtained from these provinces, the list of mortality would doubtless have been much larger; as it is, the number of deaths is perfectly appalling, and the subject merits consideration, with the view of providing, if possible, some remedy.

He has roughly classified the deaths under the headings of the snakes that inflicted the fatal wound; but the records are rather vague on this point, and the information not perhaps always very reliable. Still they are sufficiently explicit to make it clear that, in order of destructiveness, the cobra (*Naja tripudians*) occupies the first place on the list; the krait (*Bungarus ceruleus*) the second place; whilst under the headings of "other snakes" and "unknown" must be included many deaths due to cobra, *Bungarus ceruleus*, *Ophiophagus*, *Daboia*, *Echis carinata*, *Bungarus fasciatus*, *Hydrophida*, and some perhaps to the *Trimeresuri*, though, as to the last, there is reason to believe that deaths from their bites are comparatively very rare.

The total number of deaths recorded therefore stands thus:—

Bengal, including Assam and Orissa	6,845
North-west Provinces	1,995
Punjab	755
Oude	1,205
Central Provinces	608
Central India	90
British Burmah	120

Total 11,416

of a population (according to Dr. Hunter) of 120,972,263, or, in round numbers, about one person in every 10,000.

This total, large as it is, we fear cannot be regarded as the real mortality in these provinces, nor may the numbers be accepted as an absolutely true indication of the relative frequency of deaths in each.

The information from which these records were framed was, though official, probably only partial and imperfect. Dr. Fayrer believes that if systematic returns could be kept, as he has suggested that they should be, by the police in every district, subdivision, and municipality, the number of deaths would be, excluding all doubtful cases, much larger. He believes also that were such information collected throughout the whole of Hindoostan, it would be found that more than 20,000 persons die annually from snake-bite.

The result of his investigations in India has been, we think, to show that, so far, no agent or antidote, as that term is commonly understood, has been found effective in neutralizing the action of snake-poison. We think it is also pretty clearly demonstrated that death is caused in most cases, at all events where a full quantity of the virus has been injected, by its action on the nerve-centres, though whether on them alone, or also on the peripheral distribution of the nerves, or on the muscles themselves, or the exact extent to which each is affected, there may be some difficulty in determining. The futility of all the methods of treatment hitherto had recourse to is probably explained by the mode of death: their inutility had long since been demonstrated by Fontana, who, ninety years ago, among other things, showed that the outward and inward use of ammonia, as well as its injection into the veins, was as powerless for good as were all other remedies.

There is apparently some analogy between the nature of the action of the cobra-virus and that of curara, death in both cases being brought about by arrest of respiration through paralysis of the respiratory apparatus.

In the case of the curara it has been demonstrated by experiment that this is due to paralysis of the peripheral distribution of the motor nerves; and it has been further shown that if respiration be continued artificially for a sufficient length of time, perfect recovery may take place, as we have ourselves observed, the poison being eliminated from the system, and not having, during its presence, so far compromised the integrity of the parts of the nervous system where it took effect as to interfere with a resumption of their functions after its removal. Now it is evident that artificial respiration and the use of any remedies that may expedite elimination, with the application of artificial warmth to sustain temperature up to the normal standard, are the measures which may be regarded as antidotal in a rational sense to this form of poisoning; and such they have proved themselves to be; for if an animal apparently dead from curara-poisoning be kept warm and artificial respiration be kept up for some hours, it will perfectly recover.

It is in the application of similar principles that we may hope to realize a similar result in cases of snake-poisoning; and it is with this object that the investigations by Dr. Lauder Brunton and Dr. Fayrer, since his return to England, of which the present paper is an instalment, have been pursued.

Our investigations so far confirm the opinion by Dr. Fayrer already recorded, that death is due to the action of the poison on the nerve-centres, to which it is conveyed by the blood with terrible rapidity when the injection of the poison takes place into a large vein like the crural or jugular. But we have not yet arrived at absolute conclusions as to the extent to which this neurotic action is carried, whether it be localized in the nerve-centres only, or whether there be, and to what extent, any action on other portions of the nerve-apparatus.

Our experiments so far, though pointing distinctly to the centres as the seat of its action, in some cases seem to imply that the nerve-periphery and perhaps even the muscles themselves are involved; but on this head, for the present, we reserve the expression of a positive opinion.

With reference to remedial measures in cobra-poisoning, we would remark that, so far as our experiments have as yet gone, artificial respiration has certainly had the effect of prolonging life; and without committing ourselves to any opinion, we would say that we would not yet abandon hope that it may, as in the case of the curara, even save it altogether. This must of course depend on, first, the nature of the action of the poison on the nerve-apparatus—that is, whether it be of a transient or permanent character. Is it, for example, like curara, which though it destroys the power of the peripheral extremity of the motor nerves during its presence, yet leaves them uninjured and capable of resuming their functions after the poison is removed (as it may be) by elimination, life being supported by artificial respiration during that process.

If so, and the cobra-poison, even though antagonistic and annihilative of the action of the nerve-centres and peripheral distribution, or of the muscular irritability itself, be only so whilst it is present, and would, if removed within reasonable time, leave the nervous apparatus or muscles in a condition to resume their operations, then, if elimination could be carried on whilst respiration is artificially sustained, we might hope to succeed eventually in cobra as in curara poisoning.

Or could we, indeed, conceive of and find any agent so subtle as to overtake and neutralize the virus whilst it is in the system, and before it should have compromised the nerve-centres or other parts, then we should have the antidote which has been so long sought for, but yet, we fear, not found*. We do not now wish to speak of the action of the cobra-virus as it

* Fontana thought he had discovered such an agent in the "pierre à cautère" (caustic potash). He says of it:—"Mais on peut point douter cependant de l'efficacité de ce remède, et on peut affirmer que la pierre à cautère est le vrai spécifique de ce terrible venin."—*Sur les Poisons*, p. 324 (Florence, 1781).

This agent has been tried in India, but has not proved of any service in cobra-poisoning.

operates secondarily on the blood, either in those cases where great vigour of the animal or smallness of the dose have enabled the creature to resist the immediate and deadly neurotic effects of the poison. Such cases are to be classed among other septicæmiæ, and are apart from that we are now discussing.

The question resolves itself into three points of inquiry:—

1st. Is the nature of the virus such that we may hope to find any agent that may overtake, neutralize, and so render it (the virus) harmless or inert?

2nd. Does the virus exert only a temporarily pernicious action on the ultimate structure of the nerve-centres or other parts of the nerve-apparatus? *i. e.* is it only inhibitory or hurtful during its presence in the blood, but if removed would leave the nerve-apparatus in a condition to resume its functions (such is curara), or does it enter into some permanent composition or union with the nerve-elements? or, 3rd, does it so modify their arrangements as to render them permanently incapable of resuming their functions, even after the poison has been eliminated, if it may be so removed, as we know other poisons may? Such, we fear, may be snake-poison!

If the first proposition be correct, then in some subtle chemical agent, or, if the second, in artificial respiration and eliminant action we may have hope of success.

If the third, what chance have we beyond that of sustaining life as long as artificial respiration be maintained? for if the nerve-apparatus be permanently injured, no resumption of its functions can take place. Whichever of these propositions be nearest the truth, there must still be a condition in which, from the smallness of the quantity of virus inoculated, recovery is possible—one in which the full lethal effect of the virus is not produced. In such cases, no doubt, remedial measures may be of avail.

The results of investigations in India have led to the conclusion, then, that death is brought about by the action of the poison on the cerebro-spinal nerve-centres, paralyzing them, and in some cases, where the quantity of virus was large and introduced into the circulation through the medium of a large vein, acting directly on the ganglia of the heart, causing arrest of its action. In those cases where the quantity of virus inoculated is smaller and of less intensity, according to the condition of the snake or its species (the poison of some genera being less active than that of others), secondary changes, though of what precise kind we are not yet prepared to say, occur in the blood itself, but allied in character to that of other blood-poisons and probably of a zymotic nature. We would merely for the present remark that, in the first class of cases, we believe that remedies or means of treatment other than those which may be of a preventive character are as yet of no avail, whilst in the second it is probable that they may be of some efficacy. So far we believe little more has been done than to go over ground that has

already been traversed by previous observers, who have come to similar conclusions that most of the reputed antidotes have been powerless, and that where there has been an appearance of success, it has depended not on any antidotal or antagonistic action of the remedy so much as on the fact that the quantity or quality of the poison was defective; and how this may be explained, Dr. Fayrer has endeavoured to prove by showing that the snake may have been exhausted, that its poison may be deficient in quantity or in quality, or that it may have wounded without inoculating sufficient of the poison to cause death, or more than to cause slight poisoning, and probably that, by a sphincteral arrangement of fibres, as pointed out by Dr. Weir Mitchell to exist in the rattlesnake, the snake may have the power of imbedding its fangs without shedding its poison at all.

Much virtue has been recently attributed to one of the oldest and most trusted of all antidotes—ammonia; but it was long ago shown by Fontana by repeated experiments that the injection of this agent into the veins, as well as its internal administration and external application, were powerless (as may be seen by reference to the following* pages of his works), so it has proved in all the experiments made with it in India. Any complete and satisfactory means of resisting, antagonizing, or eliminating the poison and of saving life are, we fear, still unknown; and it is in the hope that by determining the physiological action of the poison we may make some advance in our knowledge of this important subject, that the following investigations have been undertaken with cobra-virus sent to us from Bengal, and of which we hope to receive continued supplies from Mr. Vincent Richards, of Balasore, who, at our request, is also carrying on a series of experiments on the subject.

Appearance and Chemical Characters of Cobra-poison.

The poison when fresh is a transparent, almost colourless fluid, of a somewhat sirupy consistence, and not unlike glycerine in its appearance. When quickly dried it forms a transparent mass of a yellowish-brown colour, and resembling some kinds of gum-arabic. The poison may be kept in a fluid state for some months without undergoing any change, but after a certain time it decomposes.

During decomposition it gives off a quantity of gas, which has been ascertained by Dr. Armstrong to be carbonic anhydride, and at the same time acquires a dark brown colour and a disagreeable odour. The dried poison may be kept for a much longer time without undergoing any apparent change.

The chemical constitution of the poison has been examined by Dr. Armstrong. He has not been able to separate from it any crystalline principle. It is partially coagulated by heat; mineral acids produce in it

* 'Traité sur le venin de la Vipère,' vol. i. pp. 108, 109, 118, 120, 124, 129; vol. ii. pp. 5, 6, 7 (Florence, 1781). 'Opusculi Scientifici,' Letter iv. pp. 125 *et seq.*

a gelatinous precipitate; absolute alcohol throws down a white gelatinous precipitate; a drop of it evaporated with a little sulphate of copper solution and then treated with caustic potash gives a violent coloration. These reactions show that the chief constituent of the poison is an albuminoid body. On an ultimate analysis being made, very little difference was found to exist between the fresh poison, the alcoholic precipitate, and the alcoholic extract. This is the only ultimate analysis of the poison of any snake which has yet been made, so far as we know. We quote the results of it, and give the composition of albumen for comparison*.

Crude poison.	Alcoholic precipitate.	Alcoholic extract.	Albumen.
Carbon, 43·55..	45·76	43·04	53·5
Nitrogen, 43·30	14·30	12·45	15·7
Hydrogen	6·60	7·	7·1
Sulphur	2·5		
Ash	traces.		

We have recently received from Bengal some cobra-poison dried and in appearance resembling dried gum. On this we hope to report on a future occasion.

Although there is little difference between the composition of the alcoholic precipitate and extract, there is an immense difference between their physiological actions, the extract being a virulent poison and the precipitate almost inert. It is to be observed that the poison examined by Dr. Armstrong had already begun to undergo decomposition; but if it should be found by further experiments that the properties of the extract and precipitate from perfectly fresh cobra-poison are the same as those of the poison he used, it will form a notable distinction between the poison of the cobra and that of the rattlesnake. The precipitate thrown down by alcohol from the poison of the rattlesnake has been ascertained to be active, while the alcoholic extract is inert (*vide* Weir Mitchell, "Physiology and Toxicology of the Venom of the Rattlesnake," Smithsonian Contributions, 1860, p. 36).

We have experimented on four different samples of poison sent from Bengal. The first was originally a clear transparent fluid; but after keeping it decomposed and became almost black, as already described.

* Dr. Armstrong in his analysis does not appear to have arrived at the same conclusions as the Prince of Canino (L. Buonaparte), who detected the presence of a peculiar principle perhaps allied to ptyaline, to which he gave the name Echidine or Viperine, in addition to fatty matter, salts, albuminous and mucous substance. It has been suggested by Prof. Busk (*vide* Holmes's 'System of Surgery,' vol. v. p. 941) that the venom may reside in a principle analogous to, though differing from, ptyaline. We would not, however, regard Dr. Armstrong's analysis as conclusive, but hope to have the result of further examination of larger quantities of the virus.

It retained its fluidity and activity to the last. The third sample was of a light-brown colour, quite solid, and resembling dry hard cheese in its consistency. The second and fourth consisted of a clear, thin, transparent fluid and a white curdy precipitate. None of these specimens had the same activity as the first; they produced similar symptoms, but much less marked.

Effects of the poison.—The local effects of the poison are partial paralysis of the bitten part, occasionally pain in it, ecchymosis around the spot where the poison has been introduced, and sometimes in other and distant parts, and, if the animal survives for some hours, infiltration and perhaps incipient decomposition of the tissues and hæmorrhagic discharge.

The general symptoms are depression, faintness, hurried respiration and exhaustion, lethargy, nausea, and vomiting. In guineapigs and rabbits peculiar twitching movements occur, which seem to represent vomiting in them, and occasionally, in fact, guineapigs do vomit. Dogs vomit, are salivated, and present an appearance as if the hair had all been rubbed the wrong way, "staring." As the poisoning proceeds paralysis appears, sometimes affecting the hind legs first and seeming to creep up the body, and sometimes affecting the whole animal nearly at the same time. There is loss of coordinating power of the muscles of locomotion.

Hæmorrhage, relaxation of the sphincters, and involuntary evacuations, not unfrequently of a sanguineous or muco-sanguineous character, often precede death, and it is generally accompanied by convulsions.

In fowls the appearance is one of extreme drowsiness; the head falls forwards, rests on the beak, and gradually the bird, no longer able to support itself, rolls over on its side. There are frequent startings, as if of sudden awaking from the drowsy state*.

The effects of the poison upon dogs, guineapigs, and rabbits are illustrated by the following experiments.

The poison which was first sent home and still remained perfectly liquid, but had become of a dark brown, almost black colour, and somewhat inspissated, was used.

Experiment I.

1.30. Three drops of this, diluted with water, were injected into the flank of a small dog. Immediately after the injection the corresponding leg was drawn up, partially paralyzed.

1.32. He walks less steadily. Tail rigidly held out.

1.35. Is restless and whining. Walks about and then sits down again. Walks unsteadily.

* In cases where the quantity of poison injected is large, and it is at the same time very active, the bitten animal small and weak, or if inoculation has taken place into a large vein, death is almost sudden, as if it were from shock. In such cases the cardiac ganglia are also probably paralyzed; at all events the heart suddenly ceases to beat.

1.45. There are distinct muscular twitches in the shoulder. General tremor.

1.47. There are twitching movements of the back.

2.8. Has been standing perfectly still. Is now pawing and licking his lips. Vomits,

2.10. Vomits again, but licks up part of what he had ejected.

2.22. Has been continually vomiting. The ejection consisted at first of food, afterwards of tenacious mucus. He now lies down apparently exhausted. He is still trying to vomit, but can bring nothing up. He tries to rise, but cannot. Convulsive struggles occur.

2.25. Breathing has ceased, but the cornea is still sensitive. Convulsive attempts to vomit.

2.27. Cornea insensible. Heart is still beating strongly. Death soon followed.

Experiment II.

A young rabbit, weighing 900 grammes, was used. An incision had been previously made through the skin of the neck and the wound again sown up, but the animal was otherwise uninjured. Two drops of cobra-poison, weighing 12 centigrammes, were diluted with 1 cubic centimetre of water.

At 4.6 the diluted poison was ejected under the skin of the left hip.

4.7. Washed out the watch-glass in which the poison had been placed with water, and injected it under the skin of the back. The animal sat quiet after the injection, occasionally licking its fore paws.

8' 30". Respiration seems hurried. The rabbit occasionally makes a jerking motion with its hind feet.

10'. Has been restless, running about, occasionally licking its fore feet.

13' 30". Still very restless, and when held makes convulsive efforts to get away. Ears are much congested.

17'. The animal is now quiet. Its ears are no longer congested.

About 20'. Quiet, with occasional starts. Disinclined to move, but can walk quite well.

25'. Movements seem difficult, and hind legs seem weak when it tries to walk.

26'. Paralysis of hind feet is increasing.

26' 15". The rabbit lays its head down on the table.

28'. When laid on its side it merely makes a few slight movements with its fore paws and then lies still. The eyes remain in a half-closed condition, and have done so for some time. When the cornea is touched the head gives a jerk, but the eyelids move very little. Respiration slow and laboured.

4.30. The chin is twitched inwards, the sternum once or twice, the hind feet at the same time being twitched backwards. The eyes open widely. Slight convulsive extension of limbs.

4.31. Respiration has stopped, cornea is insensible; thorax opened

immediately. There were large extravasations of blood under the skin of abdomen and thorax, and under the skin of the left hip. Heart beating vigorously.

The muscles contracted on direct irritation. The foot twitched when the sciatic nerve was exposed and irritated by an interrupted current. The peristaltic movements of the intestine were active after the abdomen was opened.

Experiment III.

Dissolved 5 milligrammes of dried cobra-poison which had collected round the stopper of the bottle containing it in $1\frac{1}{2}$ cubic centimetre of water, and injected it under the skin of the left hip of a guineapig, weighing 790 grammes.

In $\frac{3}{4}$ of a minute after the injection the animal became restless and uneasy and began to cry.

$1\frac{1}{2}$ minute it began to give little starts.

$3\frac{1}{4}'$. The starting motions became greater, the hind quarters of the animal being jerked upwards, and the chin drawn in towards the body; continues to cry.

$4\frac{1}{4}'$. Passes water.

7'. Less restless.

15'. Washed out the watch-glass in which the cobra-poison had been placed with about $\frac{1}{2}$ a cubic centimetre of water, and injected it as before. Immediately afterwards the restlessness increased.

24'. Seems to be trying to vomit.

27'. It cannot walk rightly.

28'. The hind legs are paralyzed and spread out laterally from beneath it.

29'. Respiration very slow and deep. The animal lies quiet, but convulsive twitches of the limb follow almost every respiration.

Respiration 8 in $\frac{1}{2}$ a minute.

30'. Cornea insensible. Respiration has ceased. Post-mortem examination made immediately. The left ventricle was much dilated, the right ventricle empty. There were two beats of the left auricle for every one of the ventricle, and the ventricular beat was weak and imperfect.

Experiment IV.

Dissolved 1 centigramme of a substance like gum, and labelled "alcoholic extract of cobra-poison," in 1 cubic centimetre of water. It dissolved easily and formed a somewhat opalescent solution.

Injected about $\frac{1}{3}$ of this (equal to $3\frac{1}{2}$ milligrammes of the dried extract) under the skin of the thigh of a rabbit weighing about a kilogramme.

4 minutes after the injection there was no apparent effect; so a similar quantity was again injected, making the total amount received by the

rabbit 7 milligrammes of extract : $5\frac{1}{2}$ minutes after the first injection the animal became very restless.

7'. Respiration rapid. The vessels of the ears were noticed to be much injected. On continuing to observe them the injection disappeared and then returned again. The alternate filling and emptying of the vessels was much more perceptible than in the normal condition. The rabbit sits quietly, but every now and then gives a start.

22'. The condition of the ears has continued the same. The eyes are becoming half shut and the eyeballs turned up.

The animal now begins to tremble. The head is laid down on the table and then raised again : this is succeeded by a nodding motion of the head. The head is next laid down on the table.

Respirations 22 in 15 seconds.

24'. The animal has sunk down on its face, and paws as if its fore legs would no longer support it. The hind legs, however, still support the posterior part of the body. Respirations 11 in 10 seconds. It seems to be trying in vain to raise its head.

26'. Respirations 8 in 10 seconds. Convulsions. The cornea is sensitive. The rabbit is now lying on its side. Respirations 5 in 15 seconds. Pulse 12 in 18 seconds.

31'. Cornea is nearly but not quite insensible. The eyeball is protruding.

About $31\frac{1}{2}'$ respiration has stopped. The heart is still beating vigorously.

32'. Cornea insensible. The animal opened immediately. The heart was beating vigorously ; 21 beats in 10 seconds.

An attempt was made to insert electrodes into the spinal cord and pass uninterrupted current through them. No effect followed ; but it is not certain that they were well in the cord. Irritation of the nerves going to the hind legs by uninterrupted current had but a slight effect. Direct irritation of the muscles caused them to contract. After the irritation was discontinued, a fibrillary twitching was observed in one of the extensions of the thigh.

42'. Heart still feebly pulsating. Irritation of the brachial, sciatic, and crural nerves has very little effect.

45'. Heart still feebly pulsating.

Experiment V.

Two drops of cobra-poison were injected under the skin of the thigh of a guinea-pig.

One or two minutes after the injection the legs of the animal began to twitch. It was then covered with a glass bell-jar.

6' after injection. The legs are again twitching. This is a peculiar motion of the hind legs, in which they seem to make an abortive attempt to kick involuntarily.

7. Respirations are deeper than usual.

9. Legs again twitching.

10. The animal is restless and moves round and round inside the bell-jar. Grunts occasionally and grinds its teeth. The hind-quarters are twitched upwards, and the nose is drawn in towards the chin at the same time.

13. Bites at the spot where the injection was made and passes water.

22. It can no longer walk.

23. It has sunk down and lies flat on the table, leaning rather to one side. Respirations are deep. There are occasional twitches of the legs.

25. Cornea is sensitive. Occasional convulsive stretches.

27. Cornea almost insensible. Respiratory movement of nostrils continues.

28. Cornea completely insensible. Post-mortem examination made immediately. The muscles of the abdomen were dark-coloured. Peristaltic movements of the intestines occurred when the abdominal cavity was opened. The heart was dark and slightly dilated; all its cavities were contracting, though feebly. There were three beats of the auricles to each one of the ventricles. Irritation of the nerves in the pelvis caused contractions of the legs.

35' after injection. The heart is still feebly contracting.

Experiment VI.

October 28th.—Injected about a grain and a half, or two grains, of the precipitate, which was thrown down from cobra-poison by alcohol, into the thigh of a guineapig.

2.30. Injection made. A few minutes afterwards it passed some milky-looking water, and then remained perfectly quiet.

3.8½. Passed water, which was quite clear.

3.33. Injected about two grains into the right femoral vein. It passed clear water almost at once.

3.35. Its nose gave a jerk inwards. Wounded leg drawn up.

3.38. Nose twitches frequently and the animal emits a faint barking sound.

3.40. Slight tremors.

3.50. Begins to eat a piece of bread placed near it.

3.58. Still twitches.

4.8. Is still sluggish, but seems nearly well. Recovered.

Experiment VII.

October 29th, 1872.—About $\frac{1}{2}$ a grain of fresh but coagulated and cheese-like cobra-poison was suspended in distilled water and injected into the back of a guineapig, weighing about a pound and a quarter.

2.23. Injection made.

2.26. The animal looks scared and is twitching. This guinea-pig is very active.

2.30. Another dose injected. The animal is twitching much. It jumped out of the deep box in which it had been placed for observation. Breathing is hurried.

2.36. It seems better. Another dose injected into the thigh.

2.45. Not much effect. Another dose injected.

2.46. Twitching continues; animal remains active. It recovered.

Means of preventing the Effects of the Poison.

There are three ways in which the toxic effects of a poison may be entirely prevented or greatly diminished. These are:—1st, by preventing its admission into the blood; 2nd, by counteracting the effects it produces while it is circulating in the body and sustaining life by artificial respiration; 3rd, by quickening its elimination. The first of these methods is the only one which has hitherto been of any great service in cases of poisoning by the bite of cobras. Various attempts have been made to counteract the effects of cobra-poison by means of antidotes; but the advantage derived from their use is still, to say the least, doubtful. No special attempts, so far as we know, have been made to hasten the elimination of the poison, or at least none have been made avowedly for this purpose, though it is possible that some of the antidotes may have had that effect. This part of the subject we will treat in a future paper.

The subject of prevention of entry of the virus by ligature or other mechanical measures has been fully discussed in the '*Thanatophidia*'; it is unnecessary to recur to it here, for the present at all events.

For the purpose of attempting to counteract the effects of the cobra-poison while it is circulating in the blood, it is necessary to have some idea of its mode of action.

Mode of Action of the Poison.

Snake-poison probably produces its fatal or deleterious effects either by completely paralyzing the nerve-centres or other portion of the nervous apparatus, and thus causing arrest of respiration, or by partially paralyzing them and also poisoning the blood, thereby inducing pathological conditions of a secondary nature, which may, according to circumstances, cause the slightest or the most dangerous symptoms.

The effect produced depends on two sets of conditions:—first, the species of the snake, its actual state at the time, the quantity and quality of its poison, and the circumstances under which it inflicts the bite; second, the species, size, and vigour of the living creature, and the circumstances under which it is bitten.

Snake-poison is essentially a neurotic, and, when it takes full effect,

it appears to kill by annihilating, in some unknown way, the source or distribution of nerve-force. It is also an irritant; for if applied to a mucous membrane or to the conjunctiva, it soon induces violent inflammation; absorption at the same time takes place, and symptoms of poisoning are produced. It is also, to a certain extent, a septic; for if the bitten creature survive, the wound and the parts about it are apt to slough and to induce septicæmia. The poison acts by absorption—that is, by entering the circulation, and so reaching the nerve-centres, it produces, according to the quantity or intensity of the venom, either death or severe local and constitutional symptoms. If it find entry by a large vein, such as the femoral or jugular, life may be destroyed in a few seconds.

The blood itself is affected by the poison.

Dr. Fayer has not been able to detect any corpuscular changes, nor has he any exact information on the chemical changes it undergoes, or may have undergone; but that it is altered there can be little doubt; and in poisoning of the lower animals, at all events by the Viperidæ, its coagulability after death is generally destroyed, whilst after death by poisoning by the colubrine snakes the blood generally coagulates*.

As the blood is the channel through which the poison acts, it is obvious that the first object should be to arrest, destroy, or prevent its entry into the circulation; or if it has already entered, to neutralize or counteract its action, or to procure its elimination by the agency of the natural depurating organs and their secretions, and to treat local, consecutive, and constitutional symptoms by such remedial measures as may be required by the patient's condition.

Absorption takes place with extreme rapidity, so fast, indeed, that it was formerly supposed, in the case of some of the more active poisons, that they acted by transmission of a shock through the nervous system; and, so far as we know at present, it is not improbable that such, in some instances, may be the case. But rapid as the effect of snake-bite sometimes is, there is no reason to believe that generally it operates on the nerve-centres through any other channel than that of the vascular system. The experiments of Blake, Hering, and, later, of Claude Bernard show that absorption takes place with such rapidity as to explain the most rapid deaths from such cause. Blake (*vide* Guy's 'Forensic Medicine,' 3rd edition, p. 388) found that a poison passed from the jugular veins to the lungs of a dog in from four to six seconds, from the jugular vein to the coronary arteries of the heart in seven seconds;

* Our experiments in England have not confirmed these observations made in India. The blood of animals dead from *Daboia*-poisoning has been found to coagulate. This is a point that needs much further and repeated observation, as, indeed, does the question of the chemistry of the blood of animals affected by snake-poison, and we hope to report further on it.

a poison injected into the jugular vein was distributed throughout the circulation in nine seconds. Claude Bernard found that a saturated solution of sulphuretted hydrogen introduced into the jugular vein of a dog began to be eliminated from the lungs in three seconds, and when injected into the femoral vein of the same dog in six seconds.

We have neither seen nor heard of any case of snake-poisoning, in man or the lower animals, so rapid (though in some Dr. Fayrer has observed the first symptoms in a few seconds) as to justify the conclusion that poisoning had occurred otherwise than through the medium of the circulation.

Some preliminary experiments made in England by one of us (Dr. Brunton) with the poison before it had undergone decomposition seemed to show that it produced paralysis of the spinal cord, of the ends of the motor nerves, and of the muscles themselves. The experiments which we made together with the same poison a few months afterwards, as well as with other samples of poison sent from India, have not given concordant results. We therefore propose to postpone the consideration of this subject to a future paper, and to confine ourselves at present to the mode in which death is produced by the poison, especially in mammals.

Somatic death, according to Bichat, may commence in the brain, lungs, or heart; but the experiments of Fontana and Legallois show that so long as circulation and respiration are kept up the body remains alive although the head be absent. The brain is only necessary to life, inasmuch as the respiratory movements cease when it is removed or destroyed, either mechanically or by the action of a poison upon it. The causes of somatic death are thus limited to failure of the circulation and failure of the respiration.

The long continuance of the cardiac pulsations after apparent death (Expts. I., III., IV., V., IX., X.) excludes failure of the circulation as the usual cause of death; and we are thus brought by exclusion to regard death caused by the bite of a cobra, or by its poison introduced into the body in any other way, as death from failure of the respiration, or, in other words, death by asphyxia. The truth of this view is well illustrated by the following experiments*, which show that the vitality of the heart may be retained for a considerable time if the respiration is kept up. It shows also that the convulsions which have been remarked by Russell and all subsequent observers as almost always preceding death are not due so much to the action of the poison itself on the nervous centres, as that they depend on the irritation which is produced in them by the venosity of the blood.

* Excepting those cases in which the poison is injected into a large vein, such as the jugular, and causes sudden arrest of the heart's action.

Experiment VIII.

July, 1872.—A drop or two of cobra-poison diluted with water was injected into the thigh of a strong fowl. Shortly after it began to droop. It then seemed drowsy, and crouched down with the beak resting on the ground: it then fell over on its side. The comb and wattles lost their bright red colour and became dusky. Almost simultaneously convulsions occurred. A cannula was quickly inserted into the trachea, and artificial respiration commenced. The comb rapidly regained its bright colour, and the convulsions ceased. On the artificial respiration being discontinued the lividity of the comb reappeared, and convulsions again began. The experiment was repeated about ten times, and on each occasion the convulsions disappeared whenever the blood became arterial, as shown by the bright colour of the comb, and reappeared when the blood became venous. After discontinuing artificial respiration, the convulsions returned and the fowl died.

Experiment IX.

November 7th, 1872.—A cannula was placed in the trachea of a rabbit.

12.57. A small quantity of cobra-poison was injected into the hip. Symptoms of poisoning came on slowly.

1.25. The animal is still breathing, but the limbs are almost completely paralyzed. Artificial respiration begun. Temperature in the rectum $101^{\circ}8$.

1.37. Paralysis is now complete. The animal is perfectly motionless, and not the slightest movement of the eyelids occurs when the cornea is touched. Temperature in rectum $100^{\circ}8$.

1.55. The animal appears quite dead, but the heart pulsates vigorously.

2.30. Cardiac pulsations as before. Temperature $98^{\circ}6$ F.

2.32. Heart as before. Temperature 97° .

4.10. Heart still beats vigorously. Temperature $95^{\circ}4$. The continuance of the artificial respiration was now entrusted to an assistant.

5. Heart beating well.

5.20. Heart beating feebly and its action jumping.

5.30. Heart beating slowly.

6.30. Heart beating a little quicker.

7.30. Heart as before.

8. Heart beating more slowly.

8.30. Cardiac pulsations are very feeble.

9.30. Very feeble and slow.

The hour was now late, the rabbit was still completely motionless, and its body felt cold to the touch. The artificial respiration was therefore discontinued, although the cardiac pulsations had not ceased. Life was evidently prolonged for some hours in this case by artificial respiration.

Experiment X.

November 28th, 1872.—One fifth of a drop of cobra-poison (the first supply), diluted with about 2 cub. centims. of $\frac{1}{2}$ per cent. salt, was injected into the external jugular of a rabbit.

12.5. Injection made.

12.20. The animal has been convulsed and paralyzed. Sensibility of the cornea has disappeared; cannula placed in trachea and artificial respiration commenced. Temperature 100° .

1.15. Temperature $96^{\circ}3$. Heart is beating vigorously.

3.13. Heart is beating as before.

3.20. In order to try if possible to quicken elimination milk was injected into the stomach.

4.5. Heart is beating as well as ever.

4.40. Heart still beating vigorously. Respiration discontinued. Death soon followed. In this case also life was prolonged by artificial respiration.

III. "Observations on the Effects of Exercise on the Temperature and Circulation." By C. HANDFIELD JONES, M.B. Cantab., F.R.S. Received May 24, 1873.

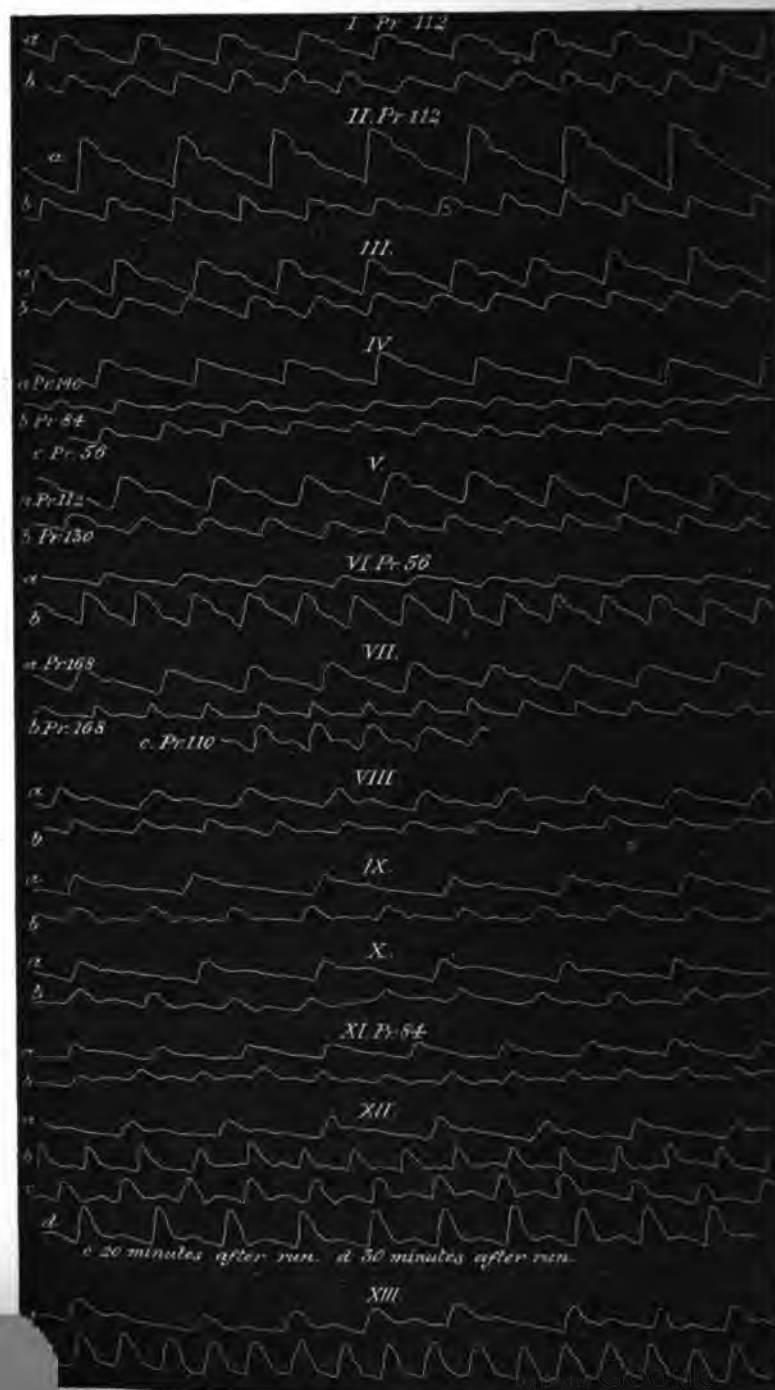
The effects of nervo-muscular exertion on the great functions of circulation and calorification in the human body must always form an interesting subject of observation, not only to the physiologist, but also to the physician, as affording the best available test of the power of the individual frame to endure the strain of disease as well as of fatigue. The effect of bodily exertion on temperature has often been observed. J. Davy found active exertion to raise the temperature $1^{\circ}7$ F. Obernier found that a quick march of $1\frac{1}{2}$ hour raised the temperature $1^{\circ}8$ to $2^{\circ}16$ F. Wunderlich gives the case of an athlete, who, while running a race, suddenly became faint and insensible, and was found soon after to have a temperature of $104^{\circ}9$ F., a pulse of 128, and some albumen in his urine. In two hours the temperature had fallen to $102^{\circ}38$. On the second morning the temperature was normal, and remained so, while the urine quickly became free from albumen. Dr. Bathurst Woodman relates the case of a youth who, after walking seventy miles in two days, had a temperature of 105° F., with a pulse of 130, and enormous quantities of lithates in the urine. Another, after similar exertion, was similarly affected. This evidence seems conclusive as to the tendency of severe nervo-muscular exertion to raise the bodily temperature; and it seems that this result is promoted by any existing debility, as in one of Obernier's observations, when the man was out of health, a very fast walk of one hour raised his temperature to $103^{\circ}28$ F. Wunderlich's and Dr. Woodman's cases are of special interest,

as showing how excessive expenditure of force may generate a condition of high but transitory fever.

My own results, so far as the temperature alone is concerned, are little more than confirmatory of those of others ; but the records of the tracings, and of the variations in the cardiac force which were noted at the same time, may add something to their value.

The observations detailed in the accompanying Tables were made on thirteen males, in average good health, during winter or spring weather, about the same hour, 8-10 P.M. The age of I. was 53-54, of II. 36, of VIII. 38, of the others 16 to 26. All bore the exertion well, except IV., who vomited, and was rather prostrate for a short time after. The exercise consisted of a smart run for a mile or two. I have described the sphygmometer with which I measured the force of the pulse in the '*Medical Times and Gazette*,' 1871, vol. ii. p. 183. After the run the pulse-rate was generally first observed, then the force necessary to compress the radial artery, then the tracing was taken, and lastly the temperature. Wherever the pressure employed in taking the tracing is not stated, it is to be understood that the same was employed before and after the run.

The result of the Sphygmometer observations was sufficiently constant to indicate the effect generally produced, or tending to be produced, by hard exercise. In eight out of the thirteen cases the artery was compressed by a less force after than it had been before the exercise. The difference in I. was represented by 100 grammes, in III. by 48, in IV. by 112, in V. by 123 (about), in VII. by 56, in VIII. by 116, in X. by 376, in XI. by 100. These figures may be considered to represent the diminution of the force exerted by the left ventricle. The amount of the arterial contraction existing at the time of each observation is perhaps in some measure concerned in the result, but is, I believe, a factor of far less importance than the cardiac contraction. This opinion is borne out by a reference to the tracings of VI., which indicate considerable arterial constriction before the run, and but little after ; yet the sphygmometer result is almost the same in both periods. In four instances, II., IX., XII., XIII., the above relation was reversed ; the force of the cardiac contraction appeared to be greater after than before the exercise. I can be sure, I think, that this did not depend on erroneous observation. The excess in II. was 56 grammes, in IX. nearly 200, in XII. 30, in XIII. 110 grammes. As these were vigorous individuals, it does not seem improbable that the exercise, which lasted only some ten or fifteen minutes, might excite the heart rather than exhaust it ; yet the tracings of II. and IX. do not bear out this view. These four instances seem to me very important, as demonstrating, what might have been suspected on other grounds, how necessary it is to apply the dynamic test in judging of the validity of the circulatory organs. It is not the amount of the force put forth by the heart at a given moment, or the correctness of the



form of the tracing at the same time, that will enable us to judge how the heart and its vessels will bear the strain of fatigue or of disease. One heart may possess much more reserve force than another, and be able to put forth an increase of energy under the very trial to which the other is succumbing. One man's arteries may possess much more tone and his capillaries more retentive power than another man's.

The Temperature observations are very accordant, showing a rise of $\cdot 2^{\circ}$ C. ($\cdot 36^{\circ}$ F.) in I. and IV., of $\cdot 3^{\circ}$ C. ($5^{\circ} 4$ F.) in VII., of $\cdot 4^{\circ}$ C. ($\cdot 72^{\circ}$ F.) in V. and IX., of $\cdot 5^{\circ}$ C. ($\cdot 9^{\circ}$ F.) in XI. and XIII., of $\cdot 6^{\circ}$ C. ($1^{\circ} 1$ F.) in VI., VIII., and X., and of 1° C. ($1^{\circ} 8$ F.) in III. and XII. The general result favours the view that the nerve-centres, which regulate temperature, are enfeebled, though but moderately, by the expenditure of force. The degree of paresis produced varies much in different individuals; but only in two of my subjects did it approach the verge of morbid. The cases, however, previously cited from Wunderlich, Obernier, and Woodman make it very probable that the same cause acting during a longer time would have produced in my instances also decided febrile phenomena; in fact it must be considered remarkable that, in spite of the free sweating which took place, the cool weather, and the lapse of some minutes which occurred before the temperature could be taken, the rise should have been so constant. One remarkable exception, indeed, there is in case II., where the exertion produced actually a fall of $\cdot 6^{\circ}$ C. ($1^{\circ} 08$ F.). This might be explained, as the individual is a man of fine physique, by a similar assumption as was made with respect to the increase of cardiac force, viz. that the regulating centres were stimulated and not depressed by the exertion; but this is doubtful, and I can find no satisfactory explanation. In observation A, at the end of the walk (twenty-two miles) the temperature was notably depressed, being no more than $35^{\circ} 4$, i. e. $1^{\circ} 2$ C. ($2^{\circ} 16$ F.) below the ordinary temperature of the individual. As food had been taken five hours before, lack of fuel could hardly have been the sole cause, as it seems to have been in Dr. Clifford Allbutt's case; in fact the individual referred to has a temperature of $36^{\circ} 2$ C. ($97^{\circ} 16$ F.) after six hours' fast, when quiescent. The tracing, observation A (δ), was small, but the circulation was fairly good, and the walker did not feel at all chilled. The contrast in this instance between the effects of short but severe exertion and prolonged moderate exertion is very marked. The former raised the temperature of the same man on one occasion $\cdot 6^{\circ}$ C. ($1^{\circ} 08$ F.), the latter depressed it just twice as much.

The Pulse-rate was doubled or more in II., IV., and V., and increased to a less amount in the others. The reason usually assigned for this acceleration, viz. that the blood arrives in greater quantity at the heart because of the pressure exerted on the veins by the contracting muscles, appears to me by no means satisfactory; for if this were the real cause, the heart's action ought to be equally accelerated, or nearly so, in all persons, irrespective of their vital condition. Such, however, is by no

means the fact; it is well known that a person in good condition will do a given piece of work with far less acceleration of pulse than one who is untrained. Thus a Londoner, in ascending a steep Surrey hill heavily weighted, had his pulse raised from 78 (while sitting still) to 120; but a countryman, of rather stronger build, performed the same work with much less acceleration. His pulse was 90 after he had walked to the bottom of the hill, 96 at the top. An anæmic patient of mine had her pulse increased from 78 to 160 by a slight exertion, which, when she was stronger and had more blood, only raised it to 103. Graves observed that the degree of acceleration of the pulse produced by sitting up in bed is a good indication of the extent to which a patient is exhausted. These facts seem to prove, *per exclusionem*, how much the state of the nerve-force has to do with the pulse-rate. Direct experiment (the section of the vagi) makes this still more certain; moreover, as acceleration of the pulse is met with very commonly in conditions of febrile debility, and as a cause of exhaustion was present in all my cases, it does not seem an unreasonable hypothesis, from all these premises, to regard weakening of the regulating action of the vagi on the cardiac nerve-apparatus as the cause of the increased pulse-rate, the vagi nerves or their centres being weakened by the withdrawal of nerve-force to the motor centres of the limbs.

But though I can hardly doubt that this view is in the main correct, harmonizing also as it does with that taken as to the cause of the increased temperature, yet it seems to be opposed by the results obtained with the manometer after division of the vagi. Dr. Sanderson finds that in such experiments the contractions of the heart are sufficiently vigorous to maintain an arterial pressure several inches higher than the normal. But if I may trust my finger and the sphygmometer, the force of the cardiac contractions and the resulting arterial pressure are generally decidedly lowered by active exertion; in fact we know that excessive exertion may produce syncope. I cannot attempt to reconcile this discrepancy. Acceleration of the pulse-rate *per se* seems to have no influence in increasing the intravascular pressure. This conclusion seems to follow from an examination of the sphygmometer records and of the tracings, which, in the majority of instances, show weakening of the cardiac force.

In judging of the Tracings, it may be assumed that in the state of quietude there is a certain amount of *nervo-muscular* force put forth by the heart and arteries with their nerve-centres, such as can be continuously and without effort produced, and that the amount exhibited in the conducting is duly proportioned to that in the impelling organs. The plan of the circulation evidently requires that the heart's force should greatly exceed that of the arteries, that the latter should yield and dilate under the pressure of the blood-current on their inner surface; and this seems very constantly to take place. Moderate exercise, which

does not exhaust the heart, as a walk, or holding up a weight for a short time, develops the tracing instead of making it smaller. This is well seen in Obs. D, E, F, G, where the greater rise implies that the artery is relaxed, but the heart not weakened. It is also well seen in cases where the exercise is more severe, as VI. and XIII., but where the heart possesses much reserve force; and is well exemplified in an interesting observation published by Dr. Fraser (*Journ. of Anat. and Phys.* 1869, iii. p. 128), where a tracing is given showing the effect of hard rowing. The after-tracing is much more ample than the one before, like VI., but differs from this in having a rounder top and a more notched fall, i. e. in showing more diastole. Where both heart and artery have been weakened, the heart seems to recover sooner than the artery; this is well shown by the tracings taken in a case where nitrite of amyl was inhaled. That taken immediately after the inhalation is irregular, and much smaller than the one taken before; while the third, taken two minutes after the inhalation, is quite remarkable for the height of its rise and the length of the fall preceding the notch. The height of the rise before the amyl was given is $\cdot 3$ inch, that immediately after is about $\cdot 175$, while the third is nearly $\cdot 6$ in. Clearly the artery yielded more on the last occasion to the impelling force of the ventricle than it had done on the first. Expenditure of force seems to produce the same effect. In obs. C, b, the tracing taken two or three hours after rowing is much ampler than that taken immediately after. In XII. the tracing taken thirty minutes after run is decidedly ampler than that taken directly after.

Severe exertion seems to exhaust the heart and render it less capable of distending the artery. M. Lortet, in his ascents of Mont Blanc, found his pulse at high elevations febrile, rapid, and so miserable that it was scarcely able to raise the spring of his sphygmograph. In most of my observations the tracing taken as soon as possible after the exercise is smaller than that taken during quiescence. This is very specially the case in IV., where the man was out of condition, and became prostrate and sick after the run; it seems, however, to be the rule, except in individuals whose hearts are gifted with more than usual staying-power, such as in VI. and VIII. Prolonged exertion, though not severe, if I may judge from one observation, also weakens the heart and diminishes the tracing.

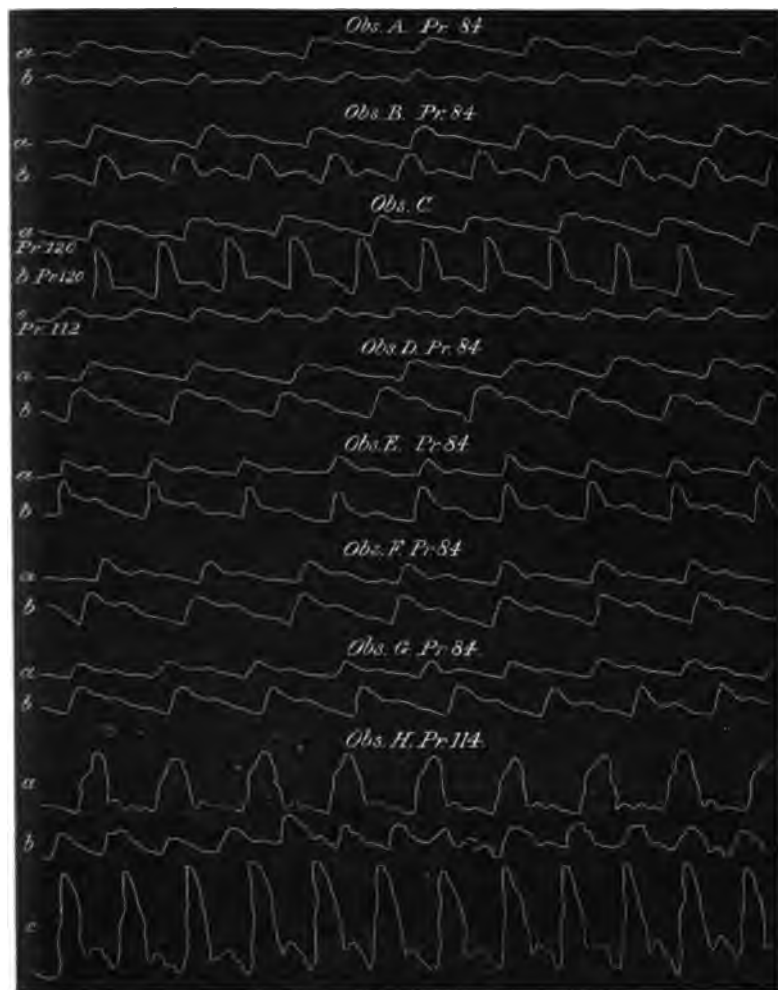
The general conclusion I am inclined to come to is, that the amplitude of the tracing depends partly on the force with which the heart is acting, and partly on the relaxation of the artery. As, however, in all cases of severe exertion there must be considerable expenditure of force, and as at the same time the blood-flow is certainly promoted in the extremities and at the surface, it can hardly be doubted that the arteries, the larger and smaller, are relaxed, and that the size of the tracing is therefore mainly dependent on the amount of cardiac force exerted, which is but slightly opposed by resistance of the arterial coat.

Cases II. and IX. are, however, a great puzzle. Here, especially in II., the tracing is more ample before than after the run; yet the sphygmometer shows that in the latter period the heart is not acting more feebly, but the reverse. This discrepancy is, I fear, not to be got over by the consideration that the height of the ascent does not depend so much on the whole amount of force put forth by the heart in a single contraction as on the amount put forth at a given instant. The artery remaining the same, a powerful contraction of the ventricle taking place gradually may no doubt produce a lower and more sloping rise than a less forcible one, which takes place more suddenly. Dr. Sanderson writes that "suddenness of contraction manifests itself in verticality and amplitude of the primary ascent of the tracing, while in those forms of pulse which correspond to a more gradual mode of contraction the first event is indistinguishable." But the heart in the cases referred to was assuredly not contracting gradually at the time when the second tracings were taken, but sharply and jerkily. Although the height of the ascent may be no measure of the increased amount of blood in the artery, yet it is, I must think, of the force applied at each systole to the inner surface of the artery and of the expansion of its tube. A sharp flick against the pad of my sphygmometer, which (the pad) is not allowed to move more than a certain distance, will drive the traveller the whole length of the scale, while a pressure less sharply and suddenly applied will not send it nearly so far. In any case, therefore, where; the artery remaining in the same state, or, *à fortiori*, having its contractility lessened, the rise is positively lowered in a tracing as compared with a previous one, I believe that weakening of the cardiac force is indicated; at least this is the only interpretation I can put on my observations. The assumption of a tightened artery would solve the difficulty above stated; but, under the circumstances, this seems so improbable that I cannot think such constriction existed.

The general conclusions as to the effect of exertion on the circulation and temperature which may be deduced from the foregoing observations are these:—

- (1) That the heart's force is, in most cases, more or less weakened by great exertion.
- (2) That the arterial contractility is probably always lessened, even when the exertion is moderate.
- (3) That after exhaustion the heart recovers sooner than the artery.
- (4) That the heart's action, in about one third of the cases of severe but brief exertion, is increased in force.
- (5) That the acceleration of the pulse probably depends chiefly on exhaustion of the vagi.
- (6) That acceleration of pulse-rate has *per se* no effect in increasing intravascular pressure.
- (7) That the temperature is usually elevated by exercise from 36° to 100.8° F., but in rare instances, or after prolonged toil, may be lowered 100.8° to 99.16° F.
- (8) That the paresis of cardiac and heat-regulating centres, coinciding with consumption of nerve-force in motor centres, shows, that, in some way one centre

is capable of drawing upon another at a time of exigency. (9) That the dynamic test is indispensable to ascertain the lasting power of the heart, the tone of the vessels, and the validity of the nerve-centres regulating the temperature; in fact to gauge the *radical* as distinguished from the *acting* forces. (10) That the capacity to endure fatigue well indicates, *ceteris paribus*, a like power to endure disease well.



Observation A.—A man, æt. 54, walked twenty-two miles, halting at the end of ten for rest and food. The tracing taken on coming in (b) was much smaller than that taken before starting (a). The temperature some minutes later was only $35^{\circ}.4$ C. ($95^{\circ}.72$ F.); an hour later, after rest and food, it was $36^{\circ}.2$ C. = $97^{\circ}.16$ F.

Observation B.—Same man. (a) Tracing taken, April 16th, while at rest; (b) tracing on return home from rowing fifteen miles, i. e. about one hour after landing. *b* presents a higher ($\cdot 15$ to 1) and more vertical rise than *a*; the first part of the fall is much steeper, the diastolic expansion is much more marked.

Observation C.—Same man. (a) Tracing taken at 1 P.M., having been at rest all the morning, May 4th, Pr. 120; (b) tracing taken May 2nd, when feeling exhausted after rowing, Pr. 120; (c) tracing taken immediately after a row, Pr. 112: *a* presents a moderate rise, a rounded top, a gradual unnotched descent; *b* a high vertical rise, a blunt but not rounded top, a descent steep in its first half, then horizontal, then rather steep again; *c* is characterized by its smallness, and its low sloping rise.

Observation D.—Same man. (a) Tracing taken while at rest, height $\cdot 1$ in.; (b) tracing taken immediately after holding up as long as possible an 8-lb. weight, height $\cdot 175$ in.

Observation E.—A youth of 18, same experiment as in D. (a) Tracing before, height $\cdot 1$ in.; (b) tracing after, height $\cdot 2$ inch.

Observation F.—Subject of D. (a) Tracing taken while at rest in evening, height $\cdot 112$ inch; (b) tracing after a walk of about 2 miles, height $\cdot 2$ inch.

Observation G.—Youth of 18, same experiment as F. (a) Tracing before walk, $\cdot 1$ inch in height; (b) tracing after, height $\cdot 175$ inch.

Observation H.—Male, æt. 62, affected with Bright's disease and anasarca. (a) Tracing before inhalation of nitrite of amyl; (b) directly after; (c) two minutes after. This experiment was suggested by Dr. Broadbent.

	Right radial compressed by. grms.	Axilla temp.	Rate of pulse.	Rate of respiration.
(I.) Before run ..	350	36.4	75	14
After „ ..	250	36.6	118	52
(II.) Before run ..	174	37	60	
After „ ..	230	36.4	120	
(III.) Before run ..	240	37	84	10
After „ ..	192	38	132	14 (taken too late)
(IV.) Before run ..	250	37.2	66	
After „ ..	138	37.4	148	36
(V.) Before run ..	261	36.8	78	21
After „ ..	138	37.2	166	
(VI.) Before run ..	180	36.6	81	20
After „ ..	192	37.2	120	51
15 min. later	174			
(VII.) Before run ..	366	36.6	78	17
After „ ..	310	36.9	126	44

	Right radial com- pressed by grms.	Axilla temp.	Rate of pulse.	Rate of re- spiration.
(VIII.) Before run ..	232	37, 98.8*	75	16
After „ ..	116	37.6, 99.8*	110	
(IX.) Before run ..	308	36.4	60	16
After „ ..	500	36.8	102	
(X.) Before run ..	540	36.6	72	
After „ ..	164	37.2	132	
(XI.) Before run ..	290	36.5	87	20
After „ ..	190	37	120	
(XII.) Before run ..	220	36.6	87	
After „ ..	250	37.6	124	
	Left radial com- pressed by			
(XIII.) Before run ..	165	36.7	60	
After „ ..	275	37.2	108	

In all the tracings *a* indicates that taken before the run ; *b*, *c*, *d* those after.

IV. "On a newly discovered extinct Mammal from Patagonia (*Homalodotherium Cunninghami*).” By WILLIAM HENRY FLOWER, F.R.S., Hunterian Professor of Comparative Anatomy, and Conservator of the Museum of the Royal College of Surgeons. Received May 30, 1873.

(Abstract.)

The author describes the complete adult dentition of a new genus of mammal, founded on remains discovered by Dr. Robert O. Cunningham in deposits of uncertain age on the banks of the River Gallegos, South Patagonia. The animal appears to have possessed the complete typical number of teeth, *i. e.* twenty-two above and below, arranged in an unbroken series, and of nearly even height, and presenting a remarkable gradual transition in characters, in both jaws, from the first incisor to the last molar. The molars more nearly resemble those of the genus *Rhinoceros* than any other known mammal ; and, judging only by the general characters of the teeth, the animal would appear to have been a very generalized type of Perissodactyle Ungulate, allied through *Hyracodon* (a North-American Miocene form) to *Rhinoceros*, also more remotely to *Macrauchenia*, and, though still more remotely, to the aberrant *Nesodon* and *Toxodon*. The generic name *Homalodotherium* was suggested for this form by Professor Huxley in his Presidential Address to the Geological Society in 1870.

* Taken by another instrument in mouth.

- V. "On the Mathematical Expression of Observations of Complex Periodical Phenomena, and on Planetary Influence on the Earth's Magnetism." By CHARLES CHAMBERS, F.R.S., and F. CHAMBERS. Received May 26, 1873.

(Abstract.)

The authors propose in this paper to determine, by Bessel's method, a mathematical expression for a periodical phenomenon from observations which are affected by one or more other periodical phenomena, and to find criteria for judging of the extent to which the expression is affected by these other phenomena: to find also the true period from observations of a single periodical phenomenon of which the period is only approximately known; or, having found an expression for a period of known approximation to the truth, to find from it the expression for the true period. In the course of these inquiries, certain ambiguities which affect similarly Bessel's expression for a single periodic phenomenon and the results here arrived at are remarked upon; and, finally, the results are applied to determine the nature of periodic planetary phenomena in a particular case.

- VI. "The Diurnal Variations of the Wind and Barometric Pressure at Bombay." By F. CHAMBERS. Communicated by CHARLES CHAMBERS, F.R.S., Director of the Colaba Observatory, Bombay. Received May 26, 1873.

(Abstract.)

The object of this paper is to bring to notice a remarkable relation that has been found to exist between the diurnal variations of the wind and the barometer at Bombay.

The observations made use of are the records of a Robinson's anemograph during the first three years of its performance, viz. from June 1867 to May 1870, and the corresponding hourly observations of the barometer and the dry- and wet-bulb thermometer made at the Government Observatory, Bombay.

The mean results for each hour of the day during the whole period and the mean diurnal variations of each element are tabulated and graphically represented by figures. The diurnal variation of the wind is then investigated, the most influential part of which is attributed to the land- and sea-breezes which blow from E.S.E. and W.N.W., and are shown to follow mainly the same law of progression as the temperature of the air, thus affording confirmatory evidence of the truth of Halley's theory of the trade-winds as applied to land- and sea-breezes.

Some peculiarities of the curve representing the land- and sea-breezes are then pointed out, and these, the writer concludes, are due to the super-

position of another distinct variation having two maxima and two minima in the twenty-four hours, like the barometer variation; and he supports his views by a reference to the variation of the east components of the wind in the months of July and August, when the land- and sea-breezes have almost disappeared. This is found to exhibit a decided *double period*. The north components of the land- and sea-breezes are then approximately eliminated from the north components of the whole variation, and the variation which then remains exhibits a very decided *double period* in this direction also. These variations with double periods are regarded as indicative of the existence of a double diurnal variation in the general movements of the atmosphere. Upon this hypothesis typical diurnal variations of the wind are deduced for north and south low latitudes—that for north latitudes exhibiting a double diurnal right-handed rotation, and that for south latitudes a double diurnal left-handed rotation; and from these the diurnal variation of the barometer is deduced.

The movements of the wind-vane at Bombay are then analyzed; and the writer concludes that the greater part of the excess of “direct” over “retrograde” rotation of the vane at Bombay is due to the *diurnal variation* of the wind.

Extracts are given from observations made at St. Helena, Toronto, and Falmouth, showing the character of the diurnal wind-variations at those places, and their greater or less agreement with the deduced typical curves. The writer maintains that these variations afford independently a possible, if not a probable, explanation of that movement of the air which Dové had called the “Law of Gyration;” and, in conclusion, he points to the extent of their applicability in deducing weather probabilities, and to the method of discussing storms.

A postscript is added, giving the mean diurnal variation of the wind at Sandwich Manse, Orkney, and pointing out its general conformity with the results deduced from the Bombay wind-observations.

VII. “Researches in the Dynamics of a Rigid Body by the aid of the Theory of Screws.” By ROBERT STAWELL BALL, LL.D. Communicated by Professor CAYLEY. Received May 29, 1873.

(Abstract.)

This paper contains some developments of a theory sketched in the *Transactions of the Royal Irish Academy*, vol. xxv. p. 157.

PART I. discusses the quantity of energy necessary to give a body a twist about one screw while acted upon by a wrench about another screw. The expression *virtual coefficient* is defined, and application is made of the reciprocal character of the virtual coefficient to solve the problem of resolving a wrench along six given screws.

PART II. Six screws of reference can be chosen, such that each screw is reciprocal to all the rest; the group is said to consist of *coreciprocal screws*. The analogy between the convenience obtained by referring the twist coordinates of a rigid body to a group of coreciprocal screws, and the convenience obtained by referring the coordinates of a point to rectangular axes, is pointed out. The important theorem that one screw can be found which is reciprocal to five given screws is discussed.

PART III. The *sexiant* is a function of six screws, which can be expressed as a determinant. The property possessed by six screws when their sexiant vanishes may be enunciated in several different ways; *e. g.*, wrenches of appropriate magnitudes equilibrate when applied about the six screws to a free rigid body. If seven twist velocities about seven screws neutralize, then each twist velocity must be proportional to the sexiant of the six remaining screws.

PART IV. If a quiescent rigid body receive an *impulsive wrench*, then the body commences to twist about an *instantaneous screw*. It is shown that if four impulsive screws lie on a cylindroid, the four instantaneous screws lie on a cylindroid, and also the four impulsive reactions caused by the constraints. The anharmonic ratios of each of these groups of four are all equal. Several special properties of impulsive and instantaneous screws are also considered.

PART V. When a body has k degrees of freedom, it is shown that k *principal screws of kinetic energy* can be determined. When an impulsive wrench is imparted about a principal screw of kinetic energy, the body commences to twist about the same screw. These principles are illustrated by detailed examination of the cases of two and three degrees of freedom.

PART VI. Miscellaneous propositions. The principal questions discussed are:—the *locus plane* of a point for twists about the screws on a cylindroid; the equilibrium of a body under the action of gravity for the different cases of freedom; remarks on Professor Sylvester's theory of lines in involution; generalization of a theorem due to M. Chasles.

VIII. "On the Fossil Mammals of Australia. Family MACROPODIDÆ. Genera *Macropus*, *Pachysiagon*, *Leptosiagon*, *Procoptodon*, and *Palorchestes*.—Part IX." By Prof. OWEN, F.R.S.
Received April 19, 1873.

(Abstract.)

In this Part the author concludes his descriptions of the fossils on hand relating to the family of Kangaroos (Macropodidæ). He gives additional evidence of the characters of *Macropus Titan*, evidence of a larger species of *Macropus* proper (*M. Ferragus*), and of two subgeneric modifications of that type (*Pachysiagon* and *Leptosiagon*). The characters of

a well-marked genus or subfamily of *Macropodidæ* are illustrated by fossils, on which are founded a genus *Procoptodon*, and the species *Proc. Pusio*, *Proc. Goliah*, and *Proc. Rapha*. The paper concludes with the description of a considerable part of a fossil cranium indicative of the largest form of kangaroo hitherto found; for the subgenus and species so indicated the author proposes the name *Palorchestes Azael*.

The illustrations of the paper form the subjects of nine 4to Plates.

IX. "Observations on the Currents and Undercurrents of the Dardanelles and Bosphorus, made by Commander J. L. WHARTON, of H.M. Surveying-Ship 'Shearwater,' between the months of June and October, 1872." From a Report of that Officer to the Hydrographer of the Admiralty. Communicated by Admiral RICHARDS, C.B., V.P.R.S. Received May 7, 1873.

There is a general flow of the Black-Sea water through the Bosphorus, Marmara, and Dardanelles to the Mediterranean, probably caused by the combination of three things:—first, the prevalence of N.E. winds in the Black Sea; secondly, the excess of water received from the large rivers over the amount lost by temperature at some seasons; and, thirdly, the difference of specific gravities in the two seas.

Of these, observation goes to prove that the wind has by far the greatest influence.

There is as general a countercurrent setting up under the surface-stream, in an opposite direction, from the Mediterranean to the Black Sea. This seems to be dependent on the surface-current; for when the latter is slack, the undercurrent is slack likewise.

DARDANELLES.

From observations made from 14th June to 30th October 1872.

The ordinary direction of the surface-current in mid-stream is S.W., or "down" the straits, with the wind, which blows from the opposite quarter for three parts of the year; but when a S.W. wind has lasted a few days and forced its way through the straits from one end to another, the current will run in the opposite direction, but never so strongly as the S.W. current.

Wherever a point juts out into the straits, or a turn occurs, counter-currents run up strongly inshore on their lee sides, as might be expected; and, generally speaking, with the exception of those places where the straits are narrowed by two points opposite to one another, either slack water or a slight eddy exists, on either shore, for a short distance from the coast.

As a general rule, the stronger and more continuous the N.E. wind, the stronger the S.W. set; but the surface-currents are capricious in their rates, the wind seeming to affect them more at some times than at others. This is probably owing to the different winds that may be blowing at the two ends of the straits, it being a common occurrence that a strong S.W. wind exists at the southern end simultaneously with a N.E. one at the northern. This was verified on several occasions, and doubtless checks the flow of water.

On calm days the current will usually be slack to the S.W.; but there are exceptions to this also, and it will sometimes even run to the N.E. It may, however, be safely stated that the current is rarely strong when there is no wind.

The general average rate of the current may be taken at $1\frac{1}{2}$ knot from one end of the strait to the other; but in some places, as at Chanak Kalehsi, it will run 3 knots, when at Gallipoli it is but 1 knot.

The maximum current is at Chanak, where, with a strong N.E. wind, it will attain a speed of $4\frac{1}{2}$ knots.

As has been stated, a south-westerly wind will stop the current in the upper part of the strait, and a long continuance will cause it to change entirely its direction, but it will never run more than a knot to the north-eastward.

Though the maximum lunar tide is only 3 or 4 inches, the water sometimes rises as much as 2 feet above its usual height, which may be accounted for by the same cause—i. e. the different winds at the two ends of the straits checking the flow below, and causing the water to accumulate above.

The observations for undercurrent have been made, as far as possible, in different kinds of weather, so as to ascertain what influence the wind might have on the movement of the water*.

If the observed rates and areas on October 4th be taken, it makes the outflow to the inflow as 3 to 1; but every correction will diminish this ratio, and it is probably nearer 3 to 2: or, in other words, the quantity of water flowing from the Marmara is half as much again as that which it receives back from the Mediterranean; but this is a very vague approximation.

* After various modifications, the drag finally adopted was a flat board 6 feet square, with a wing at right angles to the centre of it 2 feet in length.

To the extremities of this short wing a sling is made fast, and to this sling the supporting line to the buoy is bent at such a point as will keep the surface of the drag vertical when the strain comes on.

It weighs 70 lbs. in air, and took 120 lbs. of lead to sink it satisfactorily.

The line was ordinary lead line. The float was an iron nun buoy, 5 feet long by 1 in diameter in the centre, capable of supporting 80 lbs.

The pulling boats used drags of a similar shape, 4 feet square, of canvas, laced to a wooden frame to prevent collapsing, which did not require such heavy weights to sink them.

On all occasions when it has been practicable the specific gravity and temperature of the water have been taken at different depths, at the same times and places as the current observations.

The general result of the undercurrent observations in the Dardanelles is, that an *undercurrent* exists whenever a surface one does, and invariably in an opposite direction.

Even with the imperfect means used, out of 12 days' observation 10 gave this result.

The speed at which the under countercurrent travels seems to depend upon that of the surface.

The highest rate actually observed in the Dardanelles was $\frac{1}{10}$ of a knot an hour, when the surface-stream in the opposite direction was also the fastest experienced, 3 knots.

When the surface-current was slack so was the undercurrent.

There was little or no deviation from this rule.

The depth of the surface-current appears to be about 10 to 15 fathoms.

Below this, the whole of the water to the bottom is generally on the move in a contrary direction.

At about 10 or 15 fathoms depth also the specific gravity of the water generally alters rapidly, approximating to the Mediterranean density below, and to that of the Black Sea above.

The temperature also changes about this point.

This shows that it is not superfluous surface-water that is running back underneath, but that it is the Mediterranean supplying the place of the lighter Marmara water.

There appears to be little or no slack water between the two currents, favourable observations having given them in opposite directions in a very few fathoms different depth.

Details of Undercurrent Observations.—Dardanelles.

On July 29th an under countercurrent was obtained in the *centre* of Gallipoli Strait at 30 fathoms. At the commencement of this observation it was calm, and the surface ran but a tenth of a knot, with a counter undercurrent of two tenths; later in the day a breeze sprang up, when the surface ran six tenths, and the undercurrent was increased to three tenths, showing an unmistakable connexion between wind and current.

Next day gave almost identical results. The current of seven tenths of a knot at the surface decreased in strength with the depth, changed its direction at about 15 fathoms, and increased in rate to the bottom, where it showed three tenths.

On August 13th five boats, equidistantly arranged across the strait, took observations with various results; but generally the surface-current ran to N.E., and the under to the S.W., reversing the usual order; all rates very slow.

The wind was S.W., light, and had been so for two days; but it was subsequently ascertained that at the south end of the Dardanelles the S.W. wind had been fresh.

On September 2nd, in the course of experiments made to compare different drags, no undercurrent was found. It was nearly calm, and the observation was confined to one spot only.

On September 10th, after 24 hours calm, succeeding to seven days strong N.E. winds, the surface-current was almost *nil*.

Five boats obtained simultaneous observations, of which the two on the Asiatic side found the surface moving to S.W., and an undercurrent to the N.E., averaging two tenths of a knot each; the other boats' drags scarcely moved. The observation was not completed.

On October 4th four boats obtained observations in a strong N.E. wind that had been blowing for five days previously.

There was a nasty sea in from the Sea of Marmara, which rendered it difficult for the pulling boats to hold their own, and caused the loss of several drags in hauling in. The results were that, at 20 fathoms and deeper, the drags went to windward; they were stationary at 15 fathoms, and above the current ran to the S.W. The swiftest rates in both currents were at the extremes, the surface and bottom; the former $1\frac{1}{2}$ knot, the latter half a knot.

This concluded the undercurrent observations at Gallipoli.

September 18th. Four boats took observations off Bourgaz Light. The wind, which had been for three days strong from the N.E., was moderate from the same direction at the commencement of the day; but before the full set of observations could be completed, it freshened to such a degree that the boats were unable to work, and had to bear up for the ship; the steam cutter even had to desist.

The result was that at 25 and 20 fathoms all drags went to windward, though slowly, against a surface-current of $2\frac{1}{2}$ knots per hour. The densities, as usual, changed rapidly between 10 and 20 fathoms.

September 24th. After a strong N.E. wind for two days, four boats tried in the narrows between Chanak Kalehsi and Kilid Bahr. The surface-stream was rapid, and the wind, moderate at the commencement, increased up to 11 A.M., when the boats, having recovered their drags with some difficulty, had to give up any further attempt at struggling against the current and sea.

The results, on the whole, were not satisfactory. With the steam cutter on the European side of the centre using the 6-foot wooden drag, the buoy went to windward at 33, 23, and 16 fathoms at the rate of $\frac{1}{10}$, $\frac{1}{10}$, and $\frac{1}{10}$ respectively, remaining stationary at 10 fathoms. Here also the densities changed rapidly. The surface-current was $2\frac{1}{2}$ knots.

The boat on the Asiatic side of the centre obtained a slight undercurrent, but was swept away herself 2 or 3 miles to leeward whilst attempting to take temperatures.

The boat on the Asiatic side obtained another undercurrent of $1\frac{1}{2}$ and $1\frac{3}{4}$ at 17 and 23 fathoms, but at 34 fathoms it again drifted slowly to leeward. Surface-current about 3 knots.

The countercurrents obtained here were at less depths than any before observed.

October 1st. Four boats together, across the western entrance to the Dardanelles from Koum Kaleh to Seddul Bahr. The wind was light and variable, but it had been blowing half a gale from the N.E. the three preceding days, and the surface-stream was strong, nearly 3 knots, but with smooth water. The results this day were curious, as instead of the under countercurrent existing from the bottom upwards to about 15 fathoms, as usual, it was found between 10 and 20 fathoms, running strongest at 15; but below 20 fathoms the water was *apparently* moving in the same direction as the surface.

Here, too, at the surface is no longer the pure sea of Marmara water; the density shows it to be mixed with the Mediterranean, probably from a counter surface-current that sometimes runs up inshore on the European side.

BOSPHORUS.

The general flow of the water is from the Black Sea to the Marmara, and at an average rate of about $2\frac{1}{2}$ knots; but the speed of the current will vary from hour to hour, and the course of the strait is so tortuous that the water runs in swirls and streams of different speeds.

Generally the current runs quicker in the afternoon than the forenoon; and as the mornings are usually calm and the N.E. breeze gains strength during the day, this seems to be attributable to that fact.

The remark made with regard to the Dardanelles currents, that, "as a rule, in a calm the currents are slack," does not, however, apply to the Bosphorus.

On two occasions during the 'Shearwater's' visits the current seemed to have ceased altogether, without any apparent cause, calms existing at the time, but in certain places (as the Devil's Current and off Seraglio Point) the water was still draining down. The Turkish fleet which lie in the strength of the current, opposite Scutari, were swung in different directions.

It is reported that in the winter season, when S.W. gales of long duration are not uncommon, the current will run the opposite way with some strength.

The current gathers strength as it advances down the strait; at the northern entrance it was never found so strong as at the southern.

A general undercurrent running counter to the surface-stream was found at both ends of the Bosphorus; in the deep channel the rate of this, as recorded by the drift of the buoy, varied from $1\frac{1}{2}$ to $1\frac{3}{4}$ of a knot, and the depth at which it was found varied from 15 to 30 fathoms. This, it must be remembered, is subject to great corrections.

Details of Undercurrent Observations.—Bosphorus.

The spot chosen for the observations at the southern end of the Bosphorus was between Seraglio Point and Leander's Tower.

Here the whole surface runs down with great velocity, with no side eddies. There is a narrow deep channel of 34 fathoms close to the Stamboul side, otherwise the depth does not exceed 20 fathoms. In this deep channel the up undercurrent was found.

On August 21st the drag * was tried in a N.E. wind, force 4, and with a surface-current of $3\frac{1}{2}$ knots. When lowered to a depth afterwards assumed to be 20 fathoms, it at once rushed violently away against the surface-stream.

On the 12th of October the steam cutter tried again in the deep channel. This day was one of those when the surface-currents in the Bosphorus were unaccountably slack and variable. At Therapia, near the north end, the water was motionless, while at Seraglio Point, where the observations were made, the current was only $1\frac{1}{2}$ knot.

The result of the observation was a counter undercurrent of one knot at a depth of 15 fathoms. This makes the surface-current much shallower than the other observations.

On the 14th October, though calm and smooth, the surface-water was rushing down at three knots an hour; observations showed an up undercurrent at 22 fathoms, but no nearer to the surface, and at that depth only at the rate of a quarter of a knot, but the water is of the Mediterranean density a few fathoms higher.

On the 15th of October the surface-current was very rapid; $4\frac{1}{2}$ knots was recorded off Leander's Tower. The results were nearly identical with the previous days. Up current was obtained in the deep channel.

The experiments at the north entrance of the Bosphorus were made off Fil Burnu, where the strait is contracted by two opposite points, and where no side eddy exists.

On the 23rd of August the drag gave in the deep channel, at 40 and 30 fathoms, an undercurrent of $1\frac{1}{10}$ and $\frac{7}{10}$ knot respectively, the surface only running $1\frac{2}{10}$ knot.

This, during all the observations, seemed to be the invariable rate of the surface-current here, wind seeming to have little effect on it.

Densities changed between 25 and 30 fathoms. At this point also (26 fathoms) was a curious stratum of cold water of 54° , the water immediately below being 62° , and that above, 60° . The next day corroborated this.

On the 17th of October, undercurrent was again found at 30 fathoms and below, but not so strong as on August 23rd. The two months' interval had made some difference in the temperatures, but little or none

* For these observations a wooden drag was employed similar in design to that before described; it weighed 115 lbs. in air, and required 175 lbs. to sink it.

in the specific gravities. The cold stratum mentioned on 23rd, 24th, and 26th August was wanting.

BLACK-SEA TEMPERATURES.

Endeavours were made to get a series of densities and temperatures in the Black Sea, in order to ascertain the conditions at all depths.

The steam cutter with much difficulty, not unattended with danger, obtained an offing of six miles on the 17th of October.

The wind was strong and the sea high, but time would not allow of choice of days, and the N.E. wind was almost permanent.

The results obtained show a uniform density of 1.012 from surface to bottom in a depth of 38 fathoms.

Temperatures showed great decrease at the bottom, and the cold stratum observed on the 23rd, 24th, and 26th of August was found here also.

X. "Normal Primary Heptyl Alcohol." By HARRY GRIMSHAW and CARL SCHORLEMMER, F.R.S. Received May 6, 1873.

One of us has shown that by oxidizing the primary heptyl alcohol from normal heptane acid is formed, which is identical with *cenanthylic acid* from *cenanthol**. *Cenanthylic acid* is therefore a normal acid; and as by the distillation of castor-oil any quantity of *cenanthol* may be obtained, this aldehyde appears to be the best starting-point for the preparation of the hitherto almost unknown normal heptyl compounds.

Some years ago Bouis and Carlet found that, by the action of zinc and acetic acid on *cenanthol*, a heptyl acetate is formed, which yielded a heptyl alcohol boiling at 165°†. If these statements be correct, this alcohol cannot be a normal compound, for its boiling-point is only 8° higher than that of normal hexyl alcohol. One of us, on repeating these experiments, did not, however, succeed in obtaining a heptyl alcohol, the chief product of the reaction consisting of high-boiling condensation-products of *cenanthol*‡.

To convert *cenanthol* into the alcohol we have therefore made use of the excellent method which Lieben and Rossi have employed so successfully for the reduction of other aldehydes.

Cenanthol was shaken with fifty times its weight of water, and sodium amalgam and an equivalent quantity of sulphuric acid were gradually added. The product of the reaction was a mixture of heptyl alcohol and another very high-boiling liquid, the separation of which could be easily effected by distillation.

The primary heptyl alcohol thus obtained is a limpid, somewhat oily liquid, possessing an aromatic odour and boiling at 175°–177°, if the

* Phil. Trans. vol. cxii. p. 111.

† Comptes Rendus, vol. 1. p. 140.

‡ Ann. Ch. Pharm. vol. cxxvi. p. 261.

whole column of mercury is surrounded by the vapour. The high boiling-point is a further proof that this alcohol, as well as *œnanthyl*ic acid, is a normal compound.

We are now acquainted with the complete series of normal alcohols up to octyl alcohol. The following Table shows that the boiling-points in this series increase very regular for each increase of CH_2 . The boiling-points which are here given have been determined, either by the whole column of the thermometer being immersed in the vapour, or, if this was not the case, the required corrections were applied :—

Table of Normal Alcohols.

	Boiling-point.	Difference.	Observer.
Ethyl alcohol, $\text{C}_2\text{H}_5\text{O}$..	78·4	—	Kopp.
Propyl alcohol, $\text{C}_3\text{H}_7\text{O}$..	97–98	19	Different observers.
Butyl alcohol, $\text{C}_4\text{H}_{10}\text{O}$..	116·0	18·5	Lieben and Rossi.
Pentyl alcohol, $\text{C}_5\text{H}_{12}\text{O}$..	137·0	21	Lieben and Rossi.
Hexyl alcohol, $\text{C}_6\text{H}_{14}\text{O}$..	157·0	20	Franchimont and Zincke.
Heptyl alcohol, $\text{C}_7\text{H}_{16}\text{O}$..	175–177	19	H. G. and C. S.
Octyl alcohol, $\text{C}_8\text{H}_{18}\text{O}$..	196–197	20	Renesse.
Mean		19·6	

XI. “On the Organization of the Fossil Plants of the Coal-measures.—Part V. *Asterophyllites*.” By W. C. WILLIAMSON, F.R.S., Professor of Natural History in Owens College, Manchester. Received May 17, 1873.

(Abstract.)

On two occasions the author directed attention, in the Proceedings of the Royal Society (vol. xx. pp. 95 & 435), to the structure of some stems which appeared to him to belong to the well-known genus *Asterophyllites*, briefly pointing out at the same time their apparent relations to a strobilus of which he had previously published figures and descriptions (Transactions of the Literary and Philosophical Society of Manchester, third series, vol. v. 1871) under the name of *Volkmannia Dawsoni*. In the present memoir he gives a detailed exposition of the various parts of the plant, including the roots, rootlets, stems, branches, leaves, and fruit, in different stages of their development. This is done chiefly in two modifications of the primary type—one from the Lower Coal-measures of Oldham in Lancashire, the other from those of Burntisland. In its youngest state, the Oldham form first appears as a mere twig, having a central fibro-vascular bundle enclosed in a double bark. The vascular bundle consists entirely of vessels which are chiefly, if not wholly, of the

reticulated type. When divided transversely, it presents a triangular section, the triangle having long narrow arms and very concave sides. The bark is already differentiated into two layers, and has its exterior deeply indented by three lateral grooves—one opposite to each concave side of the vascular triangle. The outer layer is prosenchymatous, with vertically elongated cells; the inner one consists of cylindrical parenchyma arranged in radial lines, the cells being also elongated vertically. As the plant grew, successive vascular layers were added exogenously to the exterior of the vascular axis. Each layer consisted of a single linear row of vessels, which were of large size opposite the concavities of the triangle, and small where they approached its several angles. The radial arrangement of those in the several growths was equally regular; they were disposed in single radiating series, new laminae being intercalated peripherally as the stem grew. These radiating laminae were separated by small medullary rays. Owing to the fact mentioned, that the laminae radiating from the concave sides of the central triangle consisted of much larger vessels than those radiating from its angles, three or four such growths sufficed to convert its concave sides into slightly convex ones, whilst a few more such additions converted the vascular axis into a solid *cylindrical* rod. At this stage its transverse sections appeared definitely divided into six radiating areas—three of large open vessels radiating from the sides of the primary triangle, and three of small ones proceeding from the sides and extremities of the angles. When these growths have thus given a cylindrical form to the vascular axis, a change takes place in its further development. Concentric growths again begin to form, but in them all the vessels are of almost equally small diameters: hence the abrupt termination of the three areas of large vessels in the younger growths produces a distinct circular boundary line, marking a special stage in the genesis of the stem. From this point the additions go on uninterruptedly, the vessels of each radiating lamina or wedge increasing slowly in size from within outwards as the stem advances towards maturity. During these further developments the bark has continued to be separated into two well-defined forms. An inner layer consists of very delicate elongated cells with square ends (prismatic parenchyma); these are seen in the transverse section arranged in radiating lines proceeding from within outwards. The outer bark consists of narrow, elongated, prosenchymatous cells, having very thick walls; at intervals, corresponding with the spaces between the successive verticils of leaves in the ordinary examples of *Asterophyllites*, we find distinct nodes where the bark expands into lenticular disks. The vascular axis passes through these nodes without undergoing any visible change, either in the position of its vascular layers or in giving off vessels to the nodes or their appendages. The thin peripheral margin of each node sustains a verticil of the slender leaves of *Asterophyllites*, of which there are about twenty-six in each verticil. The aspect, dimensions, and arrangements of these

leaves correspond exactly with what is seen in the ordinary specimens found in the coal-shales. Transverse sections of them exhibit a single thick central midrib, but no traces of vascular tissues have hitherto been found in them.

The laminae of the vascular axis are separated by numerous medullary rays of small size; these rarely exhibit more than four or five cells in any vertical series, and usually but one or two. The exterior of the bark is deeply indented in each internode by three very deep superficial grooves, each one of which occupies the side of the stem corresponding with a concavity of the central triangle of the vascular axis. These grooves, which are sometimes double instead of single, extend from node to node, but do not indent the nodal disks. Owing to the great depth to which these penetrate the bark, they give a very characteristic tripartite aspect to each transverse section of these stems.

The Burntisland type agrees with the Lancashire one in all its leading features of structure and growth; but its vessels are all barred instead of being reticulated, and the author has not met with such beautiful examples of its nodal disks as he has done in the case of the other form, neither has he seen its leaves attached. On the other hand, he has found specimens of much larger diameter than any that have hitherto been detected in Lancashire, exhibiting the characteristic peculiarities already referred to in an exquisitely beautiful manner. The author has also obtained one section from this locality in which a branch is given off. The vessels of this divergent organ are derived from the central portion of one of the segments of small vessels, seen in the transverse sections, which proceed from one of the angles of the central triangle.

Having elucidated the details of the aerial stems, the author proceeds to examine such organs of fructification as appear to belong to these plants, commencing with the *Volkmannia Dawsoni*, which he described at length in the Transactions of the Philosophical Society of Manchester in 1871. This is a verticillate strobilus with a central vascular axis, of which latter transverse sections exhibit a close correspondence with the triangular bundle of *Asterophyllites*, being also triangular, with concave sides and truncate angles. But in order to adapt this primary fibro-vascular bundle to the requirements of the fruit, each of the truncate angles is enlarged, so as to make the entire section an almost hexagonal one. This axis is surrounded, as in *Asterophyllites*, by a double bark—an outer prosenchymatous one, and an inner one of more delicate cellular structure. At each node this bark expands into a lenticular disk fringed with stiff narrow bracts, which extend upwards and outwards beyond the sporangia. The latter rest upon the bractiferous disks and the basal portions of the bracts, each verticil being fertile. The sporangia are closely packed in about three concentric circles, and attached by sporangio-phores originating from each side of the base of each bract. The sporangia have cellular walls; they are full of large spores, each of

which has its surface prolonged into a number of very long radiating spines. This fruit the author unhesitatingly identifies with the aërial stems previously described.

He then examines various so-called *Volkmannia* found in the Lancashire Carboniferous shales, of which the internal structure is not preserved, but which, being found with leaves attached to them, admit of no doubt as to their belonging to *Asterophyllites*. These are regarded as being identical with *Volkmannia Dawsoni*; hence the author accepts the latter fruit as giving the internal organization of the ordinary *Asterophyllitean* strobilus. The fruit, which has been previously described by Binney, Carruthers, and Schimper, under the names of *Calamodendron commune*, *Volkmannia Binneyi*, and *Calamostachys Binneyana*, is then investigated. The above authors had associated it with *Calamites*; but its internal structure is shown to have nothing in common with that type; it consists of alternating verticils of barren and fertile appendages. The former are nodal disks bearing protective leaves; the others are verticils of sporangiophores, usually six in each verticil, and which closely resemble those of the recent *Equisetaceæ*; they project at right angles from the central axis, and expand at their outer extremities into shield-like disks, which sustain a circle of sporangia on the inner surface of each shield. The sporangia consist of a very peculiar modification of spiral cells; they are filled with spores which have been described as provided with elaters, like those of *Equisetum*; but the author rejects this interpretation, regarding the so-called elaters as merely the torn fragments of the ruptured mother cells in which the true spores have been developed. The vascular axis is shown to be *solid*, and without any cellular elements, being wholly different from that of *Calamites*, in which the vascular axis is a *hollow* cylinder containing an immensely large, cellular, and fistular pith. In one fine example of *Calamostachys Binneyana* the author has found the central fibro-vascular bundle surrounded by an exogenous ring. This, too, exhibits no resemblance whatever to the corresponding growths of *Calamites*; on the other hand, it corresponds closely with conditions occurring in some parts of *Asterophyllites*, with which group the author believes the fruit to be related, notwithstanding the peculiarity of its sporangia and sporangiophores. The author is confirmed in his conclusion that this fruit is not *Calamitean* by his having already described the structure of a true *Calamitean* strobilus, from an example in which the central axis retains most accurately the arrangements of tissues characteristic of *Calamitean* stems (Manchester Transactions, 1870). A type of stem to which the author had previously assigned the provisional generic name of *Amyelon* is now shown to be the root or subterranean axis of *Asterophyllites*, specimens being described in which clusters of rootlets are given off, in irregular order, from various points of the exterior of the branching roots. The latter have no medulla; but in the centres of several of them the author

finds the peculiar triangular fibro-vascular bundle so characteristic of *Asterophyllites*; and in all remains of the same trifid origin of the vascular layers may be traced in the peculiar curvatures assumed by the vascular laminae as they proceed from within outwards. The bark consists of two layers: the inner one is composed of ordinary parenchymatous cells, often of considerable size; the outer one consists of irregular piles or columns of cells, disposed perpendicularly to the surface of the bark, and with their tangential septa in close contact and in parallel planes. The lateral or radial boundaries of these piles of cells are more strongly defined than the transverse septa. In tangential sections of this outer bark, each of these radially disposed columns of parallel-sided cells appears as a single thick-walled parenchymatous cell, whose aspect, in common with that of its neighbours, is that of ordinary coarse parenchyma. Such sections exhibit no indication of the radial elongation of these cells seen in radial and transverse ones. On reexamining the inner bark, we discover the explanation of these appearances. Many of the larger and more peripheral of the cells of the latter are seen to be undergoing division by the development within their walls of secondary cell-partitions, which are parallel with those of the radially disposed columns. It appears obvious that each of the latter was primarily one of the cells of the inner bark, which has become elongated radially, and at the same time divided into a linear series of compressed cells by the growth of a succession of secondary divisions, all of which were more or less tangential to the periphery of the stem.

The author directs special attention to the genetic activity of this inner bark; the cells of its inner surface were obviously instrumental in producing the successive circumferential additions to the primary vascular axis, whilst those of its outer surface increased the diameter of the outer bark in the way just described.

After comparing these plants with living forms, the conclusion is arrived at that the nearest parallel to the structure of their stems is to be found in *Psilotum triquetrum*; whilst their general affinities are regarded by the author as Lycopodiaceous rather than Equisetaceous. The exogenous aspect of their successive vascular growths is, if possible, more conspicuous than in most of the other Carboniferous Cryptogams.

The structure of the stems described is identical with that of those found at Autun by Prof. Renault, and assigned by him to *Sphenophyllum*; thus the close affinity of this genus with *Asterophyllites* appears to be finally established. The *Calamites verticillatus* of authors is probably the arborescent stem of one of these plants.

XII. "On a tendency observed in Sun-spots to change alternately from the one Solar Hemisphere to the other." By WARREN DE LA RUE, D.C.L., F.R.S., BALFOUR STEWART, LL.D., F.R.S., and BENJAMIN LOEWY, F.R.A.S. Received June 12, 1873.

1. Hitherto in our reductions we have summed up the spotted areas of the various groups occurring on the sun's surface on any day, and have regarded their sum as a representation of the spot-activity for that day. It has occurred to us to see what result we should obtain by taking instead for each day the excess of the spotted area in the one solar hemisphere above that in the other.

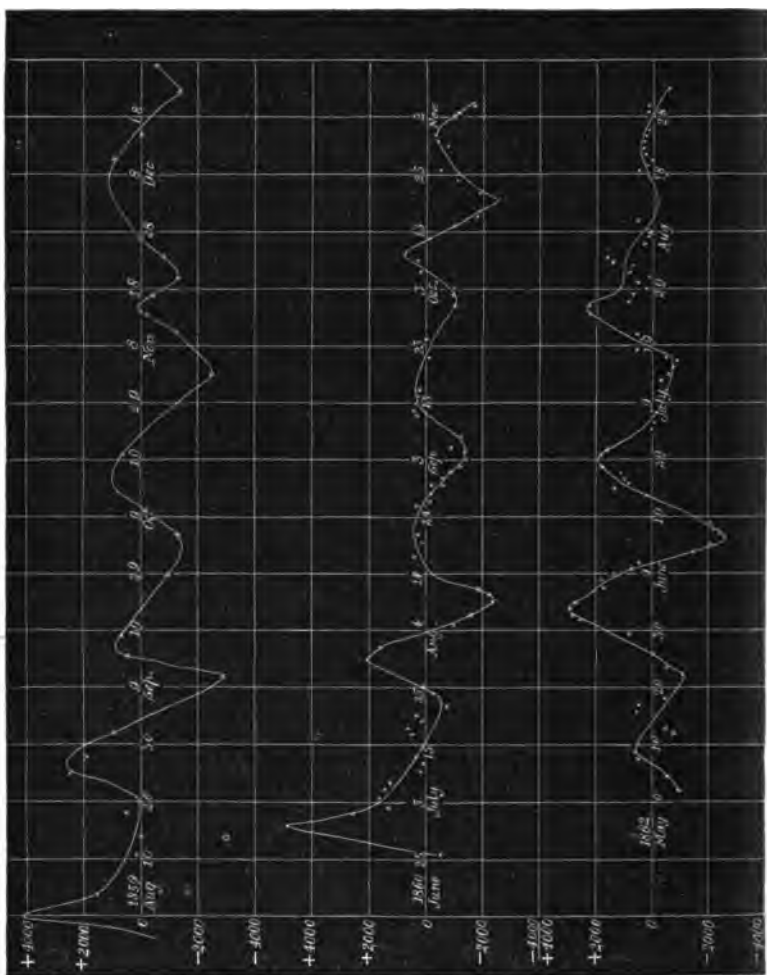
2. On adopting this method, it soon became evident that during periods of great disturbance there is a tendency in spots to change alternately from the north or positive to the south or negative hemisphere, and *vice versa*, the period of such change being about 25 days. When, on the other hand, the solar disturbance is inconsiderable, the spots do not present any such systematic oscillation.

3. We have graphically represented on the annexed diagram the results derived from this method during three of the most considerable periods of solar disturbance.

In this diagram the observed values of hemispherical excess are marked with an asterisk, and a curve is drawn so as to equalize their smaller irregularities. The northern hemisphere is reckoned positive, and the southern negative. The unit of area is, as before, the one millionth of the sun's visible hemisphere.

4. The first of these three periods extends from the beginning of August to the end of December 1859. We derive from our diagram the following Table, exhibiting the maximum amounts of hemispherical excess, with their respective dates :—

Date.	Hemispherical excess.	
	North.	South.
1859, July 31	+4180	
Aug. 18		(+ 40)
Aug. 27	+2580	
Sept. 11		—2920
Sept. 17	+ 920	
Oct. 3		—1420
Oct. 16	+1000	
Nov. 3		—2480
Nov. 15	+ 120	
Nov. 20		—1320
Dec. 7	+1050	
Dec. 22		—1400



From these we derive the following values of a period of oscillation, by taking the differences in dates between the positive extremes :—

27 days, 21 days, 29 days, 30 days, 22 days. Mean, 25·8 days ;

while doing the same with the negative extremes, we obtain

24 days, 22 days, 31 days, 17 days, 32 days. Mean, 25·2 days.

5. The second of the three periods extends from the end of June to the beginning of November 1860. Treating this in the same manner, we obtain :—

Date.	Hemispherical excess.	
	North.	South.
1860, July 1	+ 4900	
July 22	— 600
July 30	+ 2040	
Aug. 9		— 2400
Aug. 21	+ 400	
Sept. 5		— 1400
Sept. 16	+ 400	
Oct. 1		— 1180
Oct. 9	+ 800	
Oct. 19		— 2560
Oct. 31	(— 380)	

From these we derive, by taking the differences in dates of the positive extremes,

29 days, 22 days, 26 days, 23 days, 22 days. Mean, 24·4 days ;
while doing the same with the negative extremes, we obtain

18 days, 27 days, 26 days, 18 days. Mean, 22·25 days.

6. The third of these three periods extends from the beginning of May to the end of August 1862. Treating this in the same manner, we obtain :—

Date.	Hemispherical excess.	
	North.	South.
1862, May 9	+ 600	
May 22		— 1160
June 3	+ 2960	
June 15		— 2600
June 29	+ 1880	
July 16		— 800
July 26	+ 2400	
Aug. 14		— 200
Aug. 23	+ 460	

Taking, as before, the distances between the positive extremes, we obtain

25 days, 26 days, 27 days, 28 days. Mean, 26·5 days ;
while from the negative extremes we obtain

24 days, 31 days, 29 days. Mean, 28·0 days.

From the whole three periods we obtain, as the most probable mean value, 25·2 days.

7. We do not profess to have discovered the cause of these oscillations, but we would nevertheless suggest that the observational facts here brought to light may perhaps be connected with two other observational facts, the one of which was first brought to light by Carrington, and the other by ourselves.

The first of these is the fact that, generally speaking, spots in the north hemisphere have much about the same latitude as those occurring at the same, or nearly the same, period in the south, both sets widening or contracting together. We may perhaps, therefore, suppose, by applying this law, that the latitude of the spots which cause the positive extremes in the above series is not greatly different from that of those which cause the corresponding negative extremes.

The second observational law is that which tells us that spots about the same period have a tendency to attain their maximum at, or near, the same ecliptical longitude. Now, if we suppose that in the foregoing three series the greatest positive extremes were caused by the positive spots attaining their greatest size, and the greatest negative extremes by the negative spots attaining their greatest size, it would follow that the two sets, positive and negative, must have taken their rise at places on the sun's surface 180° of longitude different from each other, inasmuch as the one set about 12 or 13 days before, or after, passed (let us say) the same ecliptical longitude as the other.

But if the positive set have the same latitude as the negative, and if the one is 180° of solar longitude different from the other, it would mean that *the two outbreaks are at opposite ends of the same solar diameter*.

This conclusion is an interesting one; but, of course, it requires to be verified by further observation before it be finally received. Meanwhile we are engaged in mapping out systematically the positions of the various outbreaks upon the sun's surface, and we shall soon, therefore, be able to find whether or not there be any truth in this conjecture.

XIII. "On the Structure and Development of the Skull in the Pig (*Sus scrofa*). By W. K. PARKER, F.R.S. Received May 17, 1873.

(Abstract.)

I have for some years past determined to concentrate my attention on some one type of Mammalian Skull, so as to be able to present to the Royal Society a paper similar to those which have already appeared on other Vertebrate Skulls. I was led to work out this MEDIUM TYPE, and not a more generalized form, such as the Guineapig (see "On the Development of the Frog's Skull," Phil. Trans. 1871, p. 203), through the circumstance of an offer from my friend Mr. Charles Stewart to put some seventy embryos of the Common Pig into my possession. In the present communication I have had the invaluable help of advice and oversight from Professor Huxley; whilst the labour of my hands has been lightened by my son, Mr. T. J. Parker, who prepared for me all the more delicate sections. The embryos ranged in size from two thirds, or less, of an inch in length, with the head only equal in size to a *sweet pea*, whilst the head of the largest specimen was the size of that of the Common Squirrel. To these

I have added young pigs at birth, and have taken as the last stage the skull of a half-grown individual.

The most important results of the present investigation may be stated as follows:—

1. In a pig-embryo, in which the length of the body did not exceed two thirds of an inch, and four postoral clefts were present, the cranio-facial skeleton was found to consist of:—(a) the notochord, terminating by a rounded end immediately behind the pituitary body.

(b) On each side of the notochord, but below it, there is a cartilaginous plate, which in front ends by a rounded extremity on a level with the apex of the notochord, while behind it widens out and ends at the free lower margin of the occipital foramen. These two plates, taken together, constitute the “investing mass” of Rathke. In this stage they send up no prolongations around the occipital foramen; in other words, the rudiment of the basioccipital exists, but not of the exoccipital or super-occipital.

(c) The large oval auditory capsules lie on each side of the anterior half of the investing mass, with which they are but imperfectly united: there is no indication of the stapes at this stage.

(d) The *trabecular* or first pair of præoral visceral arches inclose a lyre-shaped pituitary space; they are closely applied together in front of this space, and, coalescing, give rise to an azygous prænasal rostrum. They are distinct from one another and the investing mass.

(e) The *pterygo-palatine* or second pair of visceral arches lie in the maxillo-palatine processes, and are therefore subocular in position. Each is a sigmoid bar of nascent cartilage, the incurved anterior end of which lies behind the internal nasal aperture, while the posterior extremity is curved outwards about the level of the angle of the mouth. The pterygo-palatine cartilages are perfectly free and distinct from the first præoral and from the first postoral arch.

(f) The *mandibular* or first pair of postoral visceral arches are stout continuous rods of cartilage which lie in the first visceral arch behind the mouth. The ventral or distal ends of these arches are not yet in contact; the dorsal or proximal end of each is somewhat pointed and sharply incurved, pushing inwards the membrane which closes the first visceral cleft and is the rudiment of the membrana tympani.

(g) The *hyoid* or second pair of postoral arches are in this stage extremely similar to the first pair, with which they are parallel. They are stout sigmoid rods of cartilage, which are separated at their distal ends, present an incurved process at their opposite extremities, and are not segmented.

(h) The *thyro-hyal* or third postoral arches, which correspond with the first branchial of the branchiate vertebrata, are represented by two short cartilaginous rods which lie on each side of the larynx.

(i) The olfactory sacs are surrounded by a cartilaginous capsule, which

has coalesced below with the trabecula of its side; while, within, the mucous membrane lining the capsule presents elevations which indicate the position of the future turbinal outgrowth of the capsule.

In this stage the posterior nares are situated at the anterior part of the oral cavity, as in the Amphibia, and the roof of the mouth is formed by the floor of the skull, the palatal plate of the maxillæ and palatine bones being foreshadowed by mere folds. The outer end of the cleft between the first and second præoral arches is the rudiment of the lachrymal duct, while its inner end is the hinder nasal aperture. The gape of the mouth is the cleft between the second præoral and first postoral arch. The auditory passage, representing the Eustachian tube, tympanum, and external auditory meatus, is the cleft between the first and second postoral arches. The proximal end of the mandibular arch, therefore, lies in the front wall, and the hyoid in the hinder wall of the auditory passage.

2. In an embryo pig, an inch in length, (a) the notochord is still visible; (b) the investing mass, the halves of which are completely confluent, has become thoroughly chondrified, and is continued upwards at each side of the occipital foramen to form an arch over it.

(c) The auditory capsules are still distinct from the investing mass, and a plug on the outer cartilaginous wall of each has become marked off as the stapes.

(d) The hinder ends of the trabecular arches have coalesced in front of the pituitary body, but they are not yet confluent with the investing mass.

(e) The pterygo-palatine rods have increased in size; they have not become hyaline cartilage, but are beginning to ossify in their centre.

(f) In the mandibular arch the proximal end has become somewhat bulbous, and is recognizable as the head of the malleus, whilst the incurved process, still more prominent than before, is the *manubrium mallei*. The rest of the arch is Meckel's cartilage; outside this a mass of tissue appears, which is converted into cartilage, rapidly ossifies, and eventually becomes the ramus of the mandible.

(g) The proximal end of the hyoidean arch, similarly enlarging and articulating with the corresponding part of the mandibular arch, becomes the incus, the incurved process attaching itself to the outer surface of the stapes and becoming the long process of the incus. The incus, thus formed out of the proximal end of the hyoidean arch, becomes separated from the rest of the arch by conversion of part of the arch into fibrous tissue, and by the moving downwards and backwards of the proper hyoid portion of the arch. A nodule of cartilage left in the fibrous connecting band becomes a styloform *interhyal* cartilage, while the proximal end of the detached arch becomes the *stylo-hyal*.

(h) The *thyro-hyals* have merely increased in size and density; they closely embrace the larynx by their upper ends.

(i) The olfactory capsules are well chondrified; their descending inner edges have coalesced with each other and, below, with the trabeculæ to form the great median septum: the turbinal outgrowths are apparent.

In this stage, the alisphenoids and orbito-sphenoids appear as chondrifications of the walls of the skull, quite separate from the investing mass and from the trabeculæ.

The floor of the pituitary space chondrifies independently of the trabeculæ and investing mass, but serves to unite these four cartilaginous tracts.

3. In an embryo pig, $1\frac{1}{2}$ inch in length, (*a*, *b*, *c*) the primordial cranium is completely constituted as a cartilaginous whole, formed by the coalescence of the investing mass and its exoccipital and superoccipital prolongations, the modified trabeculæ, the subpituitary cartilage, the auditory capsules, and alisphenoidal and orbito-sphenoidal cartilages, and the olfactory capsules. The notochord is yet to be seen extending in the middle line from the hinder wall of the pituitary fossa (now the "*dorsum sellæ*") to the posterior edge of the occipital region.

(*d*) The trabecular arches form the sides of the sella turcica, the presphenoid, and the base of the septum between the olfactory capsules; in front, where they form the azygous "*prænasal*," they are developed backwards as "*recurrent bands*," elongations of their free recurved "*cornua*."

(*e*) The pterygo-palatine arches, still increasing in size, but not chondrifying, are rapidly ossifying; they are half-coiled laminæ bounding the posterior nasal passages.

(*f*) The mandibular arch and the rudimental ramus have become solid cartilage, and the latter is ossifying as the dentary; the distal part of each mandibular rod unites with its fellow for some distance.

(*g*) The hyoid arches are each fully segmented as incus, with its "*orbicular*" head, interhyal, stylo-hyal, and cerato-hyal.

(*h*) The thyro-hyals are merely larger and denser.

(*i*) The olfactory capsules have the turbinal outgrowths all marked out as alinasal, nasal, and upper, middle, and lower turbinals.

4. In pigs of larger size the form and proportions of the parts of the cranium become greatly altered, and ossification takes place on an extensive scale, but no new structure is added.

5. It follows from these facts that the mammalian skull, in an early embryonic condition, is strictly comparable with that of an Osseous Fish, a Frog, or a Bird at a like period of development, consisting as it does of

(*a*) A cartilaginous basicranial plate embracing the notochord, and, like it, stopping behind the pituitary body.

(*b*) Paired cartilaginous arches, of which two are præoral, while the rest are postoral.

(*c*) A pair of cartilaginous auditory capsules.

(*d*) A pair of cartilaginous nasal capsules.

Further, that in the Mammal, as in the other Vertebrata the development of the skull of which has been examined, the basicranial plate grows up as an arch over the occipital region of the skull, and coalesces with the auditory capsules, laterally, to give rise to the primordial skeleton of the occipital, periotic, and basisphenoidal regions of the skull. The trabeculae become fused together, and, uniting with the olfactory capsules, give rise to the presphenoidal and ethmoidal parts of the cranium; and the moieties of the skull thus resulting from the metamorphosis of totally different morphological elements become united and give rise to the primordial cranium.

As in the Salmon and Fowl, the second pair of præoral arches give rise to the pterygo-palatine apparatus; in the Frog this arch is late in appearance, and is never distinct from the trabecular and mandibular bars, serving as a conjugational band between them. The mandibular arch, which in the Salmon becomes converted into Meckel's cartilage, the os articulare, the os quadratum, and the os metapterygoideum, in the Frog into Meckel's cartilage and the quadrate cartilage (which early becomes confluent with the periotic capsule), in the Bird into Meckel's cartilage, the os articulare, and the os quadratum (which articulates movably with the periotic capsule), in the Pig is metamorphosed into Meckel's cartilage and the malleus, which is loosely connected with the tegmen tympani, an outgrowth of the periotic capsule.

Meckel's cartilage persists in the Fish and in the Amphibia, but disappears early in the Bird, and still earlier in the Mammal. The permanent ossifications of the mandible are all membrane-bones in Fish, Frog, and Fowl, but in the Mammal (exceptionally) the ramus has a cartilaginous foundation. The hyoidean becomes closely united with the mandibular arch, and then segmented, in the Fish, into the hyo-mandibular, the stylo-hyal, cerato-hyal, and hypohyal—the hyo-mandibular, or proximal segment, articulating with the outer wall of the periotic, and many of the segments of the arch becoming dislocated.

In the Frog, the hyoid also becomes segmented, but only after extensive coalescence with the mandibular arch. The proximal segment becomes the suprastapedial (hyo-mandibular) with its extrastapedial process, and, extending inwards as mediostapedial and interstapedial, articulates with the stapes, developed by segmentation from the outer wall of the auditory capsule. The stylo-hyal is dislocated and becomes connected with the auditory capsule below the stapes (opisthotic region).

In the Bird, the hyoidean arch remains distinct from the mandibular. Whilst in its primordial condition it coalesces by its incurved apex with the auditory capsule in front of the promontory, before the stapedial plug is segmented. It then chondrifies as three distinct cartilages—an incudal, a stylo-hyal, and, distally, a cerato-hyal. The stapes becomes free from the auditory capsule, but remains united with the cartilaginous part of the incus (mediostapedial); the ascending part is largely fibrous

(suprastapedial), and the part loosely attached to the mandibular arch is the elongated extrastapedial. The short stylo-hyal afterwards coalesces with the body of the upper or incudal segment by an after-growth of cartilage (the *interhyal* tract); a long membranous space intervenes between it and the glossal piece (cerato-hyal.) Thus the "columella" of the Bird is formed of one periotic and three hyoidean segments.

In the Pig, the hyoidean arch is distinct, but articulates closely with the mandibular; its upper segment (hyo-mandibular) is converted into the incus, and becomes connected with the stapes. The stylo-hyal is dislocated and coalesces with the opisthotic region of the auditory capsule.

XIV. "Results of the Comparisons of the Standards of Length of England, Austria, Spain, United States, Cape of Good Hope, and of a second Russian Standard, made at the Ordnance Survey Office, Southampton." By Lieutenant-Colonel A. R. CLARKE, C.B., R.E., F.R.S., &c., under the direction of Major-General Sir HENRY JAMES, R.E., F.R.S., &c., Director-General of the Ordnance Survey. With a Preface and Notes on the Greek and Egyptian Measures of Length by Sir HENRY JAMES. Received May 21, 1873.

(Abstract.)

The following account of the results of the Comparisons of the Standards of Length of England, Austria, Spain, United States, Cape of Good Hope, and of a second Russian Standard at the Ordnance Survey Office has been drawn up by Lieutenant-Colonel Clarke, and is a sequel to the abstract of the results of the Comparisons of the Standards of Length of England, France, Belgium, Prussia, Russia, India, and Australia which the Royal Society has done us the honour to publish in the Philosophical Transactions for 1867, vol. clvii. p. 161.

The accurate determination of the lengths of the various standards employed by so many nations in the measure of the bases of their triangulations, which are now being united into one vast network of triangles, covering the whole of Europe, can scarcely fail to be of great importance for the advancement of physical science. To the comparison of these lengths I have added the result of our endeavours to recover the correct lengths of the most ancient measures of length with which we are acquainted, viz. those of Ancient Egypt, not only because our own measures are obviously derived from them, but also because we thus obtain the accurate relative value of the measures and distances given in the most ancient works on Astronomy and Geodesy which have come down to us.

The Ancient Egyptians employed two measures of length, viz. the common and the royal cubits.

1st. As regards the *common cubit*, we have the statement of Herodotus that the Egyptian cubit was equal to the Greek cubit, "that of Samos;" and we learn from the measurements of the Hecatompedon at Athens, by Mr. Penrose, that the Greek foot was equal to 1·013 foot, or 12·156 inches, and consequently the Greek cubit was equal to 1·520 foot, or 18·240 inches.

2nd. The most recent measures of the base of the First or Great Pyramid, that of King Cheops, viz. those made by the Royal Engineers and Mr. Inglis, a civil engineer, give a mean length of 9120 inches, or 500 cubits of 18·240 inches for the side of the square base, or 750 Egyptian feet, each Egyptian foot being equal to 1·013 English foot.

3rd. The Second Pyramid, according to the measures of Colonel Howard Vyse and Mr. Perring, has a base of 707·5 feet square, or $700 \times 1·011$ feet.

4th. The Third Pyramid has a base, according to Vyse and Perring, of 354·5 feet, or 350 Egyptian feet square, of 1·013 English foot exactly.

We may therefore confidently assume that 1·013 foot was the true length both of the ancient Greek and the ancient common Egyptian foot, and that the length of the *common Egyptian cubit* was 18·240 inches.

We have in the British Museum a double *royal cubit*, found in the ruins of the Temple of Karnak in Egypt; and I found its length to be 41·40 inches, and that of the single cubit consequently 20·70 inches, or 1·725 foot.

The pyramid which stands in the middle of the three, before the Great Pyramid (that of the daughter of King Cheops), has a base, according to Vyse and Perring, of 172·5 feet square, and therefore 100 royal cubits square exactly.

But the same authors give the breadths of no less than seven of the passages in the pyramids, including the entrances to the First, Second, and Third Pyramids, all of 41·5 inches (two cubits of 20·750 inches).

Doursther, from the measures of the nilometer at Elephantine and of three or four cubits found in the ruins of Memphis, which almost exactly correspond with each other, estimated the length of the royal cubit at 20·721 inches (see Condée, 'Dictionnaire des Poids et Mesures').

Looking to these facts, and feeling it almost certain that the common and the royal cubit had some definite relation to one another, like that between the link and foot of our own country (66 feet equal 100 links), I infer that the most probable length of the *royal cubit* was 20·727 inches, and that 88 *royal cubits* were equal to 100 *common cubits* of 18·240 inches.

This does not admit of rigid demonstration. The dimensions of Vyse and Perring seem to be given to the nearest half inch, and the measures of length sold in this country differ from one another as much as the length of the double cubit in the British Museum differs from its estimated length.

XV. "Researches on Emeralds and Beryls.—Part I. On the Colouring-matter of the Emerald." By GREVILLE WILLIAMS, F.R.S. Received May 9, 1873.

A considerable amount of discussion has taken place at various times regarding the cause of the colour of the emerald. Klaproth concluded from his earlier analyses that it was due to iron*; but the results of his later experiments†, made after he became aware of Vauquelin's discovery of the presence of chromium in emeralds‡, confirmed the observations of that chemist.

From the time of Vauquelin's analyses, the colour of the emerald was always regarded as due to the presence of oxide of chromium, until the publication of the memoir of Lewy§, who, having burnt emeralds in oxygen in a similar apparatus to that employed by M. Dumas in his researches on the atomic weight of carbon, ascertained that they contained that element, and concluded that the colour was due to the presence of some organic substance. Lewy also affirmed that the deepest-tinted emeralds contained the most carbon. The small quantity of chromium contained in emeralds he considered to be insufficient to account for the colour. Wöhler and Rose||, on the other hand, having exposed emeralds to a temperature equal to the fusing-point of copper for one hour without their losing colour, and also having fused colourless glass with minute quantities of oxide of chromium and obtained a fine green glass, considered chromium and not organic matter to be the cause of the colour.

Boussingault¶, in the course of an investigation of the "morallons"**, arrived at the same conclusion as Wöhler and Rose; and although admitting them to contain carbon, denied that it was the cause of their colour, inasmuch as they endured heating to redness for one hour without loss of colour. This result has been confirmed by Hofmeister††, who found that an emerald endured a red heat for hours without destruction of the colour, except at the edges, and concludes this small bleaching to arise from the destruction of the crystalline character of the stone. I have carefully repeated and extended these experiments. The emeralds employed were canutillos from Santa Fé de Bogota; they were kindly given to me by Professor Church. The following values were obtained in a determination of their specific gravity:—

* Klaproth, *Chem. Essays*, vol. i. p. 325 (London, 1801).

† Klaproth, *loc. cit.* vol. ii. p. 172 *et seq.* (1804).

‡ Vauquelin, *Ann. de Chim.* vol. xxvi. [1] p. 262 (1798).

§ *Comptes Rendus*, vol. xlv. p. 877 (1857).

|| *Chem. News*, vol. x. p. 22.

¶ *Comptes Rendus*, vol. lxix. p. 1249 (1869).

** The emeralds from the mines of New Granada are divided, according to Boussingault, into classes, two of the most important being the "canutillos," or finely crystallized, and the "morallons," or amorphous emeralds.

†† *Journ. für prakt. Chem.* vol. lxxvi. p. 1 (1859).

Specific gravity of Emeralds (Canulillos) before fusion.

No. of experiment.	W.	W'.	t.	ρt .	D.
I.	4.4964	2.8293	18.5	.998921	2.69
II.	4.4061	2.8294	17.0	.998841	2.69
III.	1.6655	1.0486	16.0	.999002	2.70

The formula used was

$$D = \rho t \frac{W}{W - W'},$$

where

W is the weight in air,

W' the weight in water,

ρt the specific gravity of water at t° ,

t the temperature of the water,

D the specific gravity.

One of the above emeralds was exposed for three hours in a platinum crucible to a bright reddish-yellow heat. At the end of the operation it was rendered opaque on the edges, but the green colour was not destroyed. This experiment completely confirms those of Wöhler and Rose and Hofmeister. It is, I think, quite evident that no organic colouring-matter could withstand such a temperature for so long a time. The announcement by Lewy that the depth of colour of emeralds is in proportion to the amount of carbon present, made it at first appear improbable that colourless opaque beryls would contain any of that element. The power of the colouring-matter to resist a red heat having, however, made me inclined to disconnect the question of the colour from that of the presence of carbon, I made experiments to determine whether beryls contained that element, and, if so, to what amount. An experiment was made at this stage of the inquiry (see p. 414), the result of which showed that the beryl analyzed* contained the same amount of carbon as Lewy's emerald. As it was just possible that the small increase in weight of the potash-tubes used by Lewy, Boussingault, and myself, in determining the carbon, might not have been really due to the absorption of carbonic anhydride, but to some volatile inorganic acid produced on heating the emeralds and beryls to redness in an atmosphere of oxygen, I felt it necessary to settle this question definitely. With this intention I burnt 1.2 grm. of beryl A in a platinum boat in a current of oxygen. The water produced was received in a U-tube filled with fragments of asbestos moistened with sulphuric acid. The carbonic anhydride was received in a Geissler's potash-

* As this beryl will be repeatedly alluded to in this paper, and especially in the second part, I shall, for convenience of reference, call it "beryl A." It was found in Ireland.

tube containing lime-water; this form of potash-tube was employed in this and the other experiments to be described further on, as it enables the operator to see whether the carbonic anhydride is all absorbed in the first bulb. The carefully purified oxygen was allowed to stream through the lime-water for half an hour to prove its freedom from carbonic anhydride. At the end of that time there was no trace of turbidity. The beryl was then heated to redness, and in a few seconds the lime-water in the first bulb of the potash-apparatus became milky. Thus not only proving the presence of carbon in a colourless beryl, but, taken in conjunction with the quantitative determinations, showing conclusively that the depth of colour is not, in this class of stones, in the ratio of the amount of carbon present.

But although demonstration had been obtained of the presence of carbon in the beryl A, it was still possible that it might have been derived from the decomposition of a carbonate. To settle this question, I arranged an apparatus in the following manner:—

A current of air from a gas-holder was sent in the direction indicated by the arrow (see p. 413); it passed through a solution of potassium hydrate in A and B. The three-necked bottle C contained lime-water, freshly prepared and perfectly clear. The current of air then passed into the three-necked flask D, containing 3 grms. of beryl A, finely levigated in an agate mortar, and covered to about one inch with pure distilled water. The flask E was empty, and served to arrest any thing which might have spirted over. The potash-apparatus F was filled to the height indicated with lime-water. The pipette G contained concentrated sulphuric acid. The arrangement being complete, a current of air was sent through for half an hour; not the slightest turbidity was found in C or F; the air was consequently free from carbonic anhydride. Its freedom from any other substance containing carbon had been previously determined by sending it mixed with oxygen, first into a red-hot combustion-tube, and then into a previously weighed potash-apparatus. After passage of the gas for half an hour, the potash-apparatus was reweighed and found to be absolutely unaltered. The purity of the air employed having thus been rigorously ascertained, the stopcock of the pipette G was turned, and sulphuric acid admitted into D until the water in the latter had become very hot; still no turbidity was observed in F. The fluid in D was then boiled with the same result. It was evident, therefore, that the carbon found in the beryl was not derived from the decomposition of a carbonate. The stopper at H was then removed, and about 4 grms. of pure recently fused acid chromate of potassium were added; there was still no turbidity observed in F for twenty minutes, during all which time the fluid in D was gently boiled. At the end of this time a cloudiness began to appear in the first bulb of F, and after half an hour in the second bulb: finally, a decided precipitate was obtained; it was collected, washed, and on analysis proved to be carbonate of calcium. The expe-

riment was repeated in a modified manner several times. It having been found that a faint turbidity in the lime-water was sometimes obtained before the addition of the beryl, it was traced to the presence of minute quantities of organic matter in the chromate and sulphuric acid. To eliminate this source of error, the chromate and acid were mixed at the commencement of the operation, and the current of air was kept up until every trace of carbonic anhydride was removed; at this point the beryl was added, and the effect noted. The results, both with emeralds or the beryl A, were, however, always precisely the same.

The apparatus was then recharged, and when half an hour's passage of the air produced no milkiness in F, 5 milligrammes of charcoal were introduced into D; in two minutes the first bulb, and in four minutes all the bulbs were rendered milky.

In another experiment, after the usual precautions, 5 milligrammes of graphite were acted on. In four minutes the first bulb, and in eight minutes all three bulbs were rendered milky.

The above experiments show, therefore, that the beryl A contains carbon, not in the state of a carbonate, but in a condition which is more slowly attacked than either free charcoal or graphite; and it is, I think, probably in the form of diamond, as has been shown to occur with the carbon contained in artificially crystallized boron*. The power of free chromic acid to attack the diamond with liberation of carbonic acid has been shown by the Messrs. Rodgers†.

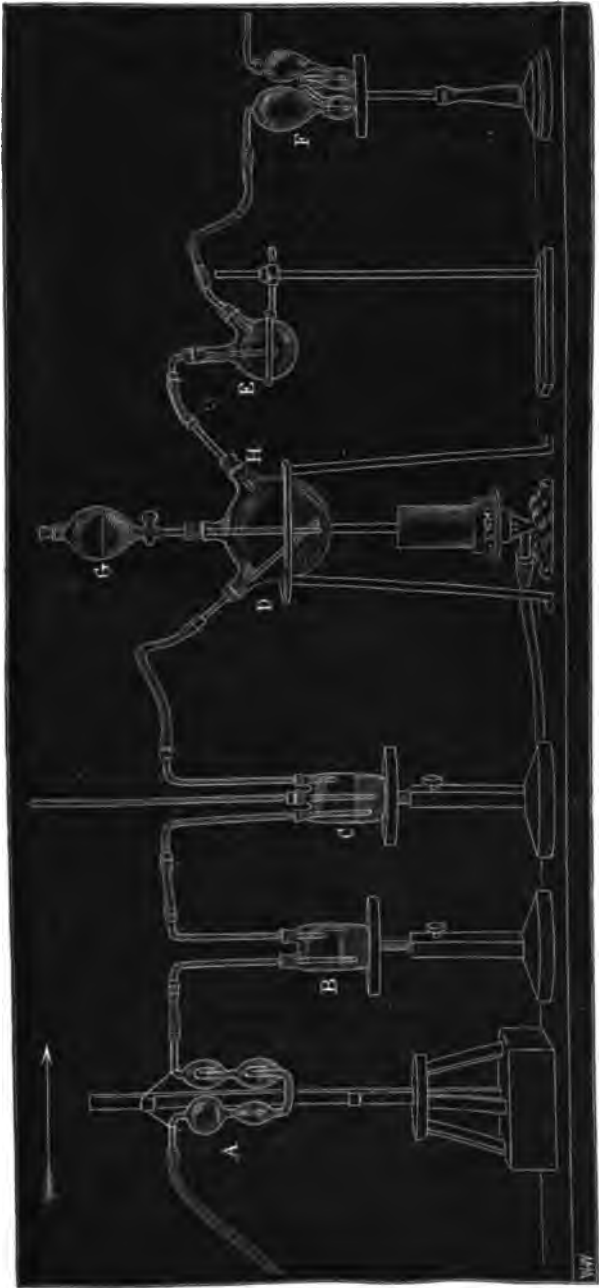
The presence of carbon in beryls does not appear to be invariable. After repeated experiments upon another large beryl from Haddam County, North America, I was unable to satisfy myself that it contained carbon. It is true that traces were found in the experiment; but they were so minute that they might have been due to the difficulty of entirely excluding the presence of organic dust during the necessary manipulations‡.

The next point I wished to ascertain was the relation borne by the quantity of carbon in the beryl A to that in the emerald. For this purpose I employed a similar apparatus to that used by Dumas in his researches on the atomic weight of carbon previously alluded to. The minute error due to the apparatus was carefully determined by going through the whole process of heating the combustion-tube to redness for the same time as in the analysis, the current of oxygen passing through at the same speed, and finally replacing the oxygen in the system of tubes by a current of pure dry air. No appreciable error was found to affect

* Wöhler and Sainte-Claire Deville, *Comptes Rendus*, February 16, 1857.

† R. E. Rodgers and W. B. Rodgers, *Chem. Gaz.* vol. vi. p. 356 (1848).

‡ Since the above paragraphs were written, an interesting paper has been published by Prof. Silliman, "On the Probable Existence of Microscopic Diamonds with Zircon and Topaz in the Sands of Hydraulic Washings in California," *Chem. News*, vol. xxvii. p. 212.



the carbon determination, but a correction had to be applied to the hydrogen. The necessity for minute precaution will be evident when it is considered that 1 grm. of beryl A, or emerald, only yielded 3 milligrammes of carbonic anhydride.

Estimation of Carbon and Hydrogen in Peruvian Emerald and Beryl A.

I.	0.9725 grm. beryl A	gave	0.0030 carbonic anhydride	and	0.0181 water.
II.	1.0082 " " "		0.0031 " "		0.0174 "
III.	1.1690 " emerald "		0.0030 " "		0.0140 "

or, per cent. :—

	Beryl A.		Emerald.	Lewy's Emerald (mean).
	I.	II.		
Carbonic anhydride	0.31	0.31	0.26	0.28
Water	1.35	1.73	1.20	1.89

In working on such minute quantities, it is not easy to speak with certainty as to the proportion of hydrogen contained in emeralds and beryls which is not due to the water present; and the difficulty is increased by the fact, insisted upon by Boussingault*, that these stones do not give off all their water below a red heat. If it be considered permissible, which I cannot admit, to calculate the hydrogen on the principle of deducting the water found on ignition in a crucible from that formed during the combustion in oxygen, and then calculating the percentage of hydrogen on the difference, as Lewy has done, the results would be as follows :—

	Beryl A.		Emerald.	Lewy's Emerald (mean).
	I.	II.		
Carbon	0.08	0.08	0.07	0.08
Hydrogen	0.06	0.11		0.04

The smallness of the values obtained, and even the very fact of their close approximation, make me offer them with a certain amount of reserve, and I shall endeavour to repeat them upon much larger quantities as soon as I have found a method of avoiding all possible sources of error.

I have not inserted the numbers given by Boussingault, as there appears to be some mistake in them. He found 0.24 per cent. of carbon in the morallons, and yet says that this number agrees with two of Lewy's determinations, one of which (he says) gave 0.21, and another 0.25; whereas I find, on reference to Lewy's memoir, that that chemist obtained in the two experiments alluded to 0.21 and 0.25, not of carbon, but carbonic anhydride.

As it was possible that some of the carbon found in these experiments might have been derived from the steel mortar used in the preliminary crush-

* *Loc. cit.*

ing of the emeralds and beryls, I pulverized some emeralds in a porcelain mortar, every precaution I could devise being taken to prevent any contact of organic matter. On burning in oxygen, and passing the products of the combustion into lime-water, a copious precipitate of carbonate of calcium was obtained.

ON THE EFFECTS OF FUSION UPON EMERALDS AND BERYLS.

On the Effects of Fusion upon Opaque Beryls.—In order to study the effects of fusion upon beryls or emeralds, I found it necessary to use the oxyhydrogen blowpipe. Beryls and emeralds were amongst the numerous substances fused with this instrument by Clarke * as long ago as 1816. He states that the Siberian beryl fuses to a clear glass containing bubbles. The Peruvian emerald he found to melt very easily to a round, extremely beautiful bead free from bubbles; it lost its green colour, and became like a white sapphire.

My first experiments were made upon the beryl A; it weighed 62.54 grms., and its density was taken with great care previous to fusion. In the first experiment the whole crystal was suspended from the balance-pan, and weighed in water; in the second a few fragments were weighed in a Regnault's flask with the usual precautions. The values obtained were as follows:—

Specific Gravity of Beryl A before fusion.

No. of experiment.	W.	W'.	t.	ρt .	D.
I.	62.5400	39.0000	21.5	.997936	2.65
II.	1.6838	1.0512	20.0	.998259	2.66

In order satisfactorily to submit beryls to the action of the oxyhydrogen blowpipe, it was necessary to find a support capable of enduring the high temperature without burning away too rapidly, and also not containing sufficient inorganic constituents to complicate the results. After a few trials I selected square prisms of gas-retort carbon, taking care to ascertain by experiment that they were adapted to the purpose. Some specimens burn away very readily, and others yield too much ash to permit of their being employed successfully. The oxyhydrogen blowpipe employed was of the ordinary construction; and, with the exception of having several nozzles of various calibres to adapt it to different quantities of the substance to be experimented upon, needs no particular description. These nozzles (which supply the oxygen) should be well formed, and free from internal irregularities. Instead of hydrogen, coal-gas was generally employed, as I have not found, for the purposes

* Schweigger's Journ. vol. xviii. p. 237 (1816).

described in this paper, hydrogen to possess advantages sufficient to counterbalance the inconvenience of storing it in the large quantities required.

The phenomena observed on submitting a fragment of beryl to the action of the flame are very beautiful; but to obtain the best results, many precautions and some little practice are necessary. The coal-gas having been lighted and the oxygen turned on, the beryl at once begins to melt, and froths or, rather, boils violently. By careful regulation of the supply of oxygen, the boiling entirely disappears; but the slightest excess of oxygen causes it to be renewed. This property of oxygen, which is found to occur with other substances besides beryls and emeralds, I hope to study in greater detail. Having so adjusted the flame that the beryl fuses tranquilly, and is yet at the exact point of maximum heat (if the substance is not too large for the apparatus), it no longer lies as a shapeless mass on the carbon support, but gathers together, rises up, and forms a perfect bead—round, clear, and brilliant. To obtain the adjustment of position necessary for this result, it is indispensable to wear very dark glasses, so dark, indeed, that objects can scarcely be discerned through them in bright daylight. Without this precaution, the minute details of the globule cannot be observed; and it would be impossible to drive away the bubbles which form instantly when the bead is moved in the slightest degree from the proper position. The heat and glare would also soon seriously affect the sight; and, with every precaution, I have found, after the preparation of one or two hundred globules, that my sight appears (even after an interval of some months) to be slightly but decidedly deteriorated. If all is working properly, the bead should be quite mobile; and advantage of this must be taken to keep it incessantly rolling, and yet not remove it from the point where it gives out the most brilliant light. By this means the whole globule is rendered transparent. If, on the other hand, it is allowed to remain without motion on the carbon (unless the globule be very minute), it will be found, when cold, to have a white opaque base, passing into the centre of the bead in a conical form, and entirely destroying its beauty. This exact adjustment of the position of the bead at the point of maximum heat, combined with constant movement of the carbon support and perfect regulation of the proportions of oxygen and hydrogen, are indispensable to the production of the glasses, specimens of which accompany this paper.

When thus fused, the globules obtained from the beryl A were clear and colourless, but generally contained a few minute air-globules and striæ, which become obvious under the lens. Towards the end of this part of the investigation I succeeded in almost entirely avoiding these defects; but I have been compelled for a time to abandon experiments in this direction in consequence of the strain thrown upon the eyes.

When chromic oxide is added to the beads, and they are again carefully fused, they acquire a fine green colour; the tint, however, is infe-

rior to that of the emerald. The green beads may, by an intense and prolonged heat, be rendered colourless. With cobalt oxide the beads afford beautiful blue glasses of any desired shade; and in all cases the results are the same as with the artificial mixture of beryl ingredients to be described further on.

The effect of fusion upon the beryl is to lessen the hardness and lower the specific gravity. The globules may be scratched by quartz. The following numbers were obtained in a determination of the specific gravity:—

Specific Gravity of Beryl A after fusion.

No. of experiment.	W.	W'.	t.	pt.	D.
I.	2.3376	1.3710	27 ^o .2	.996603	2.41
II.	2.3376	1.3699	27.0	.996603	2.41

The beryls have, therefore, lost nine per cent. of their density in passing from the crystalline to the vitreous state.

I was desirous of carefully comparing this loss of density undergone by beryls with that of rock-crystal fused under the same circumstances. According to an experiment quoted by Forbes*, the specific gravity of quartz of undoubted aqueous origin, and also that from granites, is 2.6, and that of rock-crystal, fused before the oxyhydrogen blowpipe to an amorphous glass, 2.2. According to the experiment of Le Royer and Dumas†, the specific gravity of rock-crystal determined at 4° *in vacuo* was 2.652. The value found by the Austrian Commission was 2.651223‡. I have repeated with great care the determination of the specific gravity of rock-crystal, both before and after fusion, with the annexed results:—

Specific Gravity of Rock-crystal before fusion.

No. of experiment.	W.	W'.	t.	pt.	D.
I.	1.9493	1.2154	21 ^o	.998047	2.65

The above number is practically identical with those of Le Royer and Dumas and the Austrian Commission. Rock-crystal fuses very readily before the oxyhydrogen blowpipe, and, if care be taken, the beads obtained are beautifully clear and free from bubbles.

* Chem. Soc. Journ., new series, vol. vi. p. 225.

† Gmelin's 'Handbook of Chemistry,' vol. iii. p. 354.

‡ Ueber das Verhältniss des Bergkrystall-Kilogrammes zum Kilogramme der K. Archive zu Paris (Wien, 1870). I am indebted to Prof. W. H. Miller for this reference.

Specific Gravity of Rock-crystal after fusion.

No. of experiment.	W.	W'.	t.	pt.	D.
I.	·4116	·2240	24 ^o	·997367	2·19
II.	·4116	·2261	25	·997120	2·21
III.	·4116	·2228	24	·997367	2·17
IV.	·3376	·1832	25	·997120	2·18
V.	·1796	·0977	11	·999655	2·19

Mean 2·19

Rock-crystal loses, therefore, no less than seventeen per cent. of its specific gravity on passing from the crystalline to the amorphous state, or about a half per cent. less than is undergone by garnets, according to the observations of Magnus; whereas the beryl A only lost nine per cent., or little more than half as much.

On the Effects of Fusion upon Emeralds.—On heating alone before the oxyhydrogen blowpipe these emeralds bear a bright red heat without losing their colour; and at a heat which causes incipient fusion, the edges turn colourless and opaque, while the centre remains green. After fusion for a short time they yield an opalescent greenish glass, which, kept for a long time at the maximum temperature of the blowpipe, becomes quite transparent and almost colourless. The addition of chromic oxide causes the bead to become of a dull green colour, which is not improved by moderate heating. The fact that emeralds endure a temperature capable of fusing the edges without the centre losing colour, appears conclusive against the idea of the colouring-matter being organic. The beads produced by the fusion of emeralds resemble those formed in the same manner from beryls; the phenomena during the fusion are also nearly alike; but it takes longer and a higher temperature to produce a colourless transparent bead with emeralds than with colourless beryls. The beads can be scratched by quartz, and the density is reduced to the same extent as with the beryl.

Specific Gravity of Emeralds (Canutillos) after fusion.

No. of experiment.	W.	W'.	t.	pt.	D.
I.	·7432	·4334	13 ^o	·999430	2·40

The density of fused emeralds is therefore almost exactly the same as the globules obtained in a similar manner from the beryl A.

Beryls, from the most various sources and of the greatest difference in appearance, vary but little in specific gravity; thus a large crystal of beryl from Haddam County, North America, weighing 1089 grms., had its specific gravity determined by suspension, and the number obtained was

2·67. The beryl A from Ireland gave 2·66; and a beautiful transparent yellow crystal, the locality of which is doubtful, gave 2·69, or exactly the same as the emeralds from Santa Fé.

On the Effects of Fusion upon an Artificial Mixture of Beryl Ingredients.
—Being desirous of trying the effects of fusion upon an artificial mixture of the same composition as that of a beryl, I made a series of careful analyses of the beryl A. The results of these analyses have led me to a laborious examination of the processes at present in use for the separation of alumina from glucina. The study of the original carbonate-of-ammonia process of Vauquelin, and the modifications of Rose, Joy, Hofmeister, and others, has taken twelve months of constant work; but even my earlier analyses enabled me to obtain a sufficiently close approximation to the composition of the beryl A. The following were the proportions used:—

Silica	67·5
Alumina	18·5
Glucina	14·0
	<hr/> 100·0

I did not introduce any iron or magnesia, as I regard them as accidental impurities varying in amount.

When a mixture of the above composition is exposed to the flame of the oxyhydrogen blowpipe, it fuses with almost exactly the same phenomena as with the natural beryl. It is, however, as might be anticipated from the absence of iron and chromium, much easier to get a colourless transparent bead with the mixture than with either emeralds or beryls. The greatest difficulty in this respect is, of course, found with emeralds. The specific gravity of the fused globules was determined with the following result:—

Specific Gravity of Artificial Amorphous Beryls.

No. of experiment.	W.	W'.	t.	pt.	D.
I.	·5774	·3394	13	·909430	2·42

or almost exactly the same as the density of native emeralds and beryls after fusion.

When a small portion of chromic oxide is added to the artificial mixture and the whole is subjected to fusion, the resulting bead is of a rich yellowish green, and in many experiments approached to the emerald tint; but, as a rule, the colour is more of a faded leaf-green; and although I have never obtained a globule of the vivid tint of a fine emerald, the glasses, when well cut, are quite beautiful enough to serve as jewels.

Prolonged heating gradually diminishes the colour, the bead gradually becoming of the palest bottle-green, and, finally, nearly colourless. This result is the same as with the emerald.

The metallic oxide which yields the finest tints when fused with opaque beryls, or the artificial mixture, is that of cobalt. The manner in which this oxide withstands the intense heat of the oxyhydrogen flame is remarkable. All tints, from nearly black to that of the palest sapphire, can be obtained, and the resulting glasses, when cut, are extremely beautiful, and have almost the lustre of crystallized gems.

The globules obtained by fusing the artificial mixture of beryl ingredients with didymium oxide show the characteristic absorption-spectrum of that metal in a very perfect manner, the lines being intensely black. Even when the bead is quite opalescent from insufficient heating, the black lines are beautifully distinct in the spectroscope. With a large quantity of didymium oxide the beads are of a lively pink, becoming more intense by artificial light, and, when cut, form very pretty gems. The presence of didymium in sufficient quantity raises the specific gravity.

Specific Gravity of Artificial Amorphous Beryls containing Didymium.

No. of experiment.	W.	W'.	t.	pt.	D.
I.	·9467	·5815	11	·999655	2·59

the resulting number being almost as high as that of the emerald before fusion.

Conclusions.—The evidence given in this paper, showing that colourless beryls may contain as much carbon as the richest-tinted emerald, taken in conjunction with the ignition experiments, and the results of the fusion of chromic oxide with colourless beryls and with an artificial mixture of the same composition, leaves me no room to doubt the correctness of Vauquelin's conclusion, that the green colour of the emerald is due to the presence of chromic oxide.

The fact that emeralds and beryls lose density when fused cannot properly be cited as proving that they have been made in nature at a low temperature; for it is quite possible that they were crystallized out of a solution in a fused mass, originally formed at a temperature high enough to keep the constituents of the emerald in a state of fusion, and that the crystals developed themselves during a slow process of cooling or evaporation. The method employed by Ebelmen* for the artificial production of chrysoberyl, namely heating alumina, glucina, and carbonate of calcium with boracic acid in a porcelain furnace until a portion of the menstruum had evaporated, yielded crystals of the true specific gravity, showing the

* Ann. Chim. Phys. [3] vol. xxii. p. 223 (1848); vol. xxxiii. p. 40 (1851).

density of minerals to be less dependent on the temperature at which they are produced than upon their crystalline or amorphous state.

One crystalline gem (the ruby) has undoubtedly been produced in nature at a high temperature. I have frequently repeated Gaudin's* experiment on the artificial formation of this stone, and can confirm most of his results. I did not, however, find the density to be quite the same as the native ruby or sapphire, which is, in different specimens, from 3.53 to 3.56. Artificial rubies of the finest colour made by me by Gaudin's process had a specific gravity of 3.45, which is not three per cent. lower than that of the ruby. The reason for this close approximation will be found in the fact that fused alumina crystallizes on cooling. The crystallization, however, is confused and imperfect, which causes the resulting product to be only partially transparent, and to have a slightly lower specific gravity than the natural gem. It is, consequently, scarcely correct to call the fused stones made by Gaudin's process "artificial rubies."

I have convinced myself that rubies have been formed in nature at a temperature equal, or nearly equal, to that of the fusing-point of alumina, from the circumstance that the reaction between chromic oxide and alumina, which results in the development of the red colour of the gem, is not effected at low or even moderately high temperatures, but requires a heat as high as that of the oxyhydrogen blowpipe. It is not necessary that the chromium should be presented to the alumina in the form of chromic acid. It appears, therefore, that the red colour of the ruby is not caused by the presence of chromic acid; it is, in fact, a reaction *sui generis* between alumina and chromic oxide, which, as far as my experiments have gone, only takes place at very elevated temperatures.

In my next communication I propose to give the results of a comparative study of two of the processes most generally employed for the analysis of emeralds, beryls, and other minerals containing glucina and alumina, namely the carbonate-of-ammonia process of Vauquelin, and the caustic-potash method devised by the same chemist, but modified by Gmelin, and generally associated with his name. These studies are already far advanced.

Specimens of various beryl glasses, cut and uncut, accompany this paper.

* Ann. Pharm. vol. xiii. p. 234.

XVI. "On a Standard Voltaic Battery." By LATIMER CLARK, C.E. Communicated by Prof. Sir WILLIAM THOMSON, F.R.S. Received June 19, 1873.

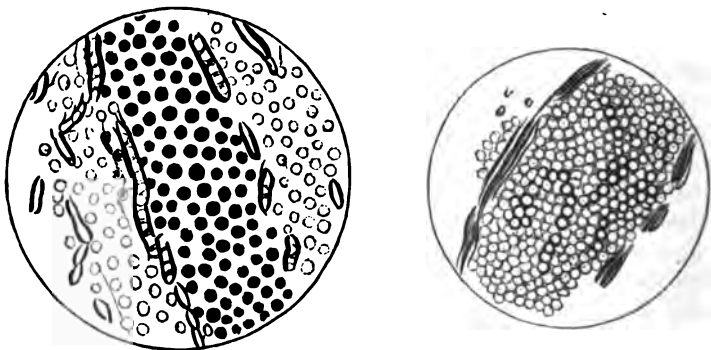
This paper is a revised version of a paper read last Session. (For Abstract see Proceedings, vol. xx. p. 444.)

XVII. "Note on High-Power Definition as illustrated by a compressed *Podura*-scale." By E. B. BEAUMONT, F.R.S., and Dr. ROYSTON-PIGOTT, F.R.S. Received June 19, 1873.

Nothing in microscopic matters has ever afforded us such complete satisfaction as the following result of a very fine definition, accomplished by means of a Gundlach German $\frac{1}{8}$ immersion lens, corrected by a new method, which Dr. Pigott at present delays publishing in the hope of further improvement, but which he is willing to exhibit at his house.

A *Podura*-slide*, fortunately strongly protected with a thick glass cover, having accidentally been subjected to so considerable a pressure as to crush out the structure of a large scale, upon bringing it, by hap-hazard, into the field of view with a magnifying-power of about 2000 diameters, exhibited a structure indicated by the woodcuts here given, and

Podura-scale.



drawn, in the presence of the writers, at Dr. Pigott's house by the accurate artist Mr. Hollick, deaf and dumb and a rapid delineator without the camera. Mr. Beaumont's surprise and admiration equalled that of Dr. Pigott. This circumstance will excite no surprise when it is stated that for four years the spherules of the *Podura* have been generally denied and warmly disputed. In ordinary cases a crushed scale shows nothing; and as glasses are usually corrected to show the illusory spines or markings, these spherules are concealed.

* *Podura Degeria vel domestica*.

The idea conveyed was, that two layers of spherules (first detected by Mr. Beaumont within the tubes), like two confined layers of small shot, had, by compression, been forced and largely spread out into broader layers. It was thought also that detached portions resembled long tubes or puckers filled with spherules exactly fitting them. The spherules appeared perfectly spherical, but somewhat unequal in size.

In the general flattened and extended surface of the compressed and disintegrated scale the spherules appeared dark blue or red, according to the slight change in the focal plane, and in a still lower plane white.

In the adjoining uninjured scales long strings of beads were seen, like necklaces of coral, here and there sharply bordered with black lines, apparently denoting tubes of membrane or puckers enclosing them like a tube. Between these strings of spherules peeped forth others of a light orange-colour.

The slide was an old one and well known. The mass of the crushed scale occupied a much broader space than any of the scales.

XVIII. "On the Accommodation of Vision, and the Anatomy of the Ciliary Body." By ARTHUR TREHERN NORTON, F.R.C.S., Lecturer on Anatomy at St. Mary's Medical School. Communicated by Dr. SIBSON, V.P.R.S. Received June 5, 1873.

(Abstract.)

This paper is to show that the increase in the convexity of the lens, when accommodated for near vision, is effected by compression of the equator of the lens by an erectile cushion composed of the ciliary processes turgid with blood, the ciliary muscle being the motor agent; also that the iris aids accommodation by increasing its rapidity, but that accommodation of vision can be effected slowly without the assistance of the iris.

The author states that, by dissection of human eyes, he has determined the existence of an erectile mass attached to the interior surface of the ciliary muscle from which the ciliary processes proceed, and that in dissections of injected specimens the vessels of the ciliary processes and of the erectile mass can be seen to pass through and between the fibres of the ciliary muscle near to the apex of that muscle.

From the festooned appearance of the ciliary processes when uninjected, and from their greatly increased size when injected, he concludes that the ciliary processes, and the mass from which they project, are erectile, and are capable of undergoing a great alteration in size, the erection being due to compression of their veins by contraction of the ciliary muscle.

In the anatomy of the ciliary muscle he gives, as the origin of that muscle, the middle fasciculus of the posterior elastic lamina of the cornea,

and as the insertion $\frac{1}{4}$ to $\frac{1}{3}$ the fibres into the connective tissue of the choroid coat of the eyeball, and the remaining fibres into the connective tissue of the erectile mass and ciliary processes.

Judging the action of the muscle from the attachment of its fibres, he states that those fibres inserting into the choroidea, and which are seen to pass around the veins derived from tissues which he terms erectile, must, when contracting, compress those veins, and so cause the erection of the ciliary processes and ciliary mass; whilst the fibres of the muscle which curve round to insert into the connective tissue of the ciliary processes, when contracting, cause those structures to approach the muscle, thereby tensing them upon their contained vessels, so as to render them solid and to prevent them from flattening upon the anterior surface of the lens.

The attachment of the ciliary muscle to the suspensory ligament of the lens, either primarily or secondarily, is denied; so that the contraction of that muscle cannot draw forward the suspensory ligament as hitherto described.

The existence of a ciliary ligament is not admitted.

The author continues:—

Accommodation is known to depend upon the convexity of the lens.

The convexity of the lens depends upon the size of the erectile organs (ciliary processes and ciliary mass), which, when the eye is accommodated for closest vision, assume the greatest amount of erection. These erectile organs form a circular cushion, which overlaps the equator of the lens; and as the size of this cushion increases as the erectile tissue becomes filled with blood, so does the cushion exert a greater amount of pressure upon the equator of the lens, decreasing the substance of the lens at its equator, and increasing it at its poles; in this manner the lens is increased in convexity and accommodated for near vision.

So far accommodation may be effected without the aid of an iris; but without an iris the change of accommodation from extreme distant to extreme near vision must occupy the time required to fill the ciliary processes with blood to complete erection. With the iris the change is almost (but not quite) instantaneous.

From the circumferential attachment the iris curtain passes obliquely forward for $\frac{1}{10}$ to $\frac{1}{4}$ inch, and then falls at a *sharp angle*. [In operations the iris tears from this angle, and not from its circumferential attachment.] The *plane* of the iris is therefore $\frac{1}{10}$ to $\frac{1}{4}$ inch in front of its attachment.

Papillary narrowing is associated with accommodation for near vision. The muscular fibres of the iris are circular and radiating; and though the two sets would appear to counteract each other, yet both can act at the same time. When the circular fibres (sphincter) contract, the radiating fibres contract with a power not sufficient to overcome the contraction of the circular fibres, but with a power sufficient to tense the

curtain of the iris and to obliterate the sharp angle already described. The plane of the iris is then thrown back to a level with the circumferential attachment.

The ciliary cushion, which is at the same time, one may say, slowly enlarging by the erection of the ciliary processes &c., and which lies behind the iris, is thus instantly pressed back by the iris upon the equator of the lens, and the lens is therefore instantly accommodated for the nearer vision. The iris is thus shown to greatly increase the rapidity of accommodation.

XIX. "On Mr. Spottiswoode's Contact Problems." By W. K. CLIFFORD, M.A., Professor of Applied Mathematics and Mechanics in University College, London. Communicated by W. SPOTTISWOODE, M.A., Treas. and V.P.R.S. Received June 19, 1873.

(Abstract.)

The present communication consists of two parts.

The first part treats of the contact of conics with a given surface at a given point; this class of questions was first treated by Mr. Spottiswoode in his paper "On the Contact of Conics with Surfaces," and general formulæ applicable to all such questions were given.

The results of that paper are here reproduced with some additions; with the exception of a few collateral theorems, these are all contained in the following Table :—

- *Number of five-point conics through fixed point = 6
- *Order of surface formed by five-point conics through fixed axis = 8
- Number of six-point conics through fixed axis = 9
- *Number of seven-point conics = 70

The second part treats of the contact of a quadric surface with a surface of the order n ; and in particular it determines the number of points at which a quadric (other than the tangent plane reckoned twice) can have four-branch contact with the surface. In his paper "On the Contact of Surfaces," Mr. Spottiswoode proves that at an arbitrary point on a surface there is no other solution than the doubled tangent plane, and gives the conditions that must be satisfied by those points at which another solution is possible.

The method here adopted is an extension of that applied by Joachimstal to the contact of lines with curves and surfaces. The coordinates of a point on a conic are expressed in terms of a single parameter, those of a point on a quadric by two parameters. To determine the intersection with a given surface we have an equation in the parameter or parameters; and the conditions of contact are expressed in terms of the coefficients of that equation. The special case of the intersection of a quadric with a cubic surface is treated by the method of representation on a plane.

* These results constitute the additions.

- XX. "Observations on the Functions of the Ganglia which are formed in the Posterior Roots of the Spinal Nerves." By ROBERT LEE, M.D., F.R.S. Received June 19, 1873.

(Abstract.)

The author gives a summary of the history of the discovery of the ganglia on the posterior roots of the spinal nerves, and expresses the intention of stating his views respecting the functions of the cervical and thoracic ganglia of the sympathetic nerve and the posterior roots of the spinal nerves in a future communication.

- XXI. "Researches in Circular Solar Spectra, applied to test Residuary Aberration in Microscopes and Telescopes, and the construction of a Compensating Eyepiece, being a sequel to the paper on a Searcher for Aplanatic Images." By G. WEST ROYSTON-PIGOTT, M.A., M.D. Cantab., Memb. Roy. Coll. Phys., Fellow of the Camb. Phil. Soc., the Royal Ast. Society, &c., and late Fellow of St. Peter's Coll., Cambridge. Communicated by Prof. STOKES, Sec.R.S. Received April 24, 1873.

The researches detailed in the present paper were commenced in May 1871. The results arrived at were largely obtained from using the microscope. Similar but less brilliant and more scanty appearances can be obtained with the telescope; but the very high power and ready adaptability of the former confers some advantages not offered by the latter.

In both, however, the same principles are illustrated.

A cone of rays of small angular aperture having the object-glass for its base in each case engages the eyepiece and emerges parallel, and the eyepieces are similar in each.

Peculiar facilities for studying solar spectra and their indications of aberrations and mechanical errors also are afforded by the former. The focal plane of vision may be employed to examine the effects of the interference of complex cones of light of large angular aperture, at least twenty times larger than those observable by the telescope.

The subject of the optical contacts of Venus at the coming transit confers peculiar interest on the nature of accurate definition of the final image presented to the eye-glass, especially as the new parallax will be entirely dependent on the keen definition of the four contacts.

The discovery by the writer of an unsuspected residuary aberration in the best microscopes, described in the Philosophical Transactions for 1870, renders it probable that some such a residuum still remains in telescopes; and this might impair the accuracy of such delicate observations as the apparent contacts between Venus and the solar limb. Eyepieces, abounding

generally with spherical aberration, require also particular attention. I have repeatedly observed a fine state of definition completely blurred merely by a change of eyepiece of the same power, which no mere focusing ameliorated, and which could only be corrected by a change in the convergent pencil passing through the objective intrinsically affecting its aberration.

Another branch of such an inquiry would be the *nature of the definition of an organic particle under high powers*, as every such research (such as the detection of the characteristics of diseased and healthy cells) may be resolved into the power of the microscope to define a single organic particle. Such particles are generally brilliant and refracting, and the errors of observation are unfortunately at present of a numerous kind. (Appendix A.)

On the Circular Solar Spectrum.

If a lens* be placed with its axis coincident with that of the microscope, and if its principal focus formed by the solar rays be examined by an instrument of the highest quality, we shall find that minute slices, as it were, of the *solar cone* present phenomena of rare beauty and order, dependent upon the quality of the examining instruments.

If two plano-convex lenses are placed with axes coincident, a good many coloured rings may be counted, but no black ones; so soon as their axes become oblique, the solar spectrum takes an intricate form, whilst the centre shows a brilliant cross (✱), very difficult to describe or represent by portraiture, but worthy of the highest photographic art of all the forms described.

This spectrum I venture to name the *circular solar spectrum*.

In my former research I had observed a flame-disk in a darkened room. This disk presented two or three diffraction-rings, similar to those of telescopic stars, but much broader.

The same method was attempted with the sun. Various objectives were used to obtain a solar miniature of the sun's disk.

Plain mirrors of glass silvered at the back entirely failed.

In order to form a pure and brilliant solar spectrum under the microscope, it occurred to me to take advantage of the principle of total internal reflection from a prism. I then constructed a prism-heliostat, which, acting in sunshine, presented an aerial miniature of the sun of great splendour (almost as dazzling as the sun itself) for half an hour, without further adjustment.

The prism-heliostat was furnished with a crown-glass double-convex lens, itself being of flint; and other lenses of less focal length could be attached to diminish or increase the diameter of the primary image of the sun, which, as the focal length generally used was 3 inches, gave an image $3 \sin 30'$ in diameter, or one fortieth of an inch nearly. In some cases object-glasses and eyepieces were placed in the solar rays emanating from the prism.

* This term of course includes every form. Of concave lenses, however, only very small ones can be conveniently examined.

Received by an inverted object-glass of the finest quality at a distance of 200 inches, the solar disk could be further miniaturized to any desirable degree of minuteness. A theoretical diameter of sixteen millionths was found convenient. To moderate the overpowering brilliance of such a spectrum directly viewed, dark slides of graduated neutral tints were at first used, and smaller primary disks were obtained by using deeper lenses at the prism.

When this minute spectrum is viewed with a high power (800 or 1000), the phenomena attending residuary errors, whether of achromatism, spherical aberration, or mechanical construction, are demonstrated with so keen a severity upon the handiworks of man as to throw all other methods into the shade.

Phenomena observed.

With the flame-disk formerly used* only two or three diffraction-rings could (as already remarked) be descried; an extraordinary number of richly coloured rings of dazzling brilliance was now exhibited at the instant of bringing the solar disk into the plane of focal vision.

The most striking feature, amid so much effulgence, was an intensely black (*jet-black*) diffraction-ring encircling the central disk at the clearest focal point. The appearance of the rings changed every instant with the slightest change of focus, and their tints indicated the nature of the "secondary spectrum."

Upon closer inspection, my curiosity was excited by observing the shape of the primary or central black ring deviating from a true circular form, somewhat squared off, as though not consisting of one pure black ring. I then found, upon more careful examination, by change in the eyepieces, length of body, and "collar corrections," that it was composed of several excentric rings. The research, as one depended upon the fitful gleams of a spring sunshine, though tedious, was at this point enlivened with the outcome of an important fact, which will be more fully noticed further on [*viz.* that achromatism and aplanatism, in the best adjustable microscopes, at present were found to be altogether incongruous].

The same result, the enlarged disk, as described in my former paper was obtained.

The theoretical disk (exhibited on the stage of the microscope) appeared increased to nearly four times its proper size†.

* Phil. Trans. part ii. 1870, p. 595.

† Diameter of disk at prism $3 \sin 30'$	= .026
Diameter of miniature at 200 inches distance, reduced by $\frac{1}{4}$ -objec-	
tive 1600 times	= 0.000016
$\frac{16}{1000000}$	
Observed diameter of solar disk, exclusive of its jet-black diffraction-	
ring	= 0.000061
Estimated breadth of black ring	= 0.00002
And 0.000016 : 0.000061 :: 4 : 1 nearly.	

Three general features were constantly observed: the rings were seen either wholly or chiefly on one side of the solar disk, i. e. either within or without the focus, or nearly similar, except in colour, on opposite sides of the focal point.

If the rings were on one side a nebulous brightness occupied the other, into which the solar disk suddenly resolved itself on a slight change of focus; but frequently this nebulosity assumed a fine-grained or "engine-turned pattern." Occasionally two primary disks, each with its own system of rings, struggled for the mastery; and on changing the focus a chromatrope effect was produced by the expanding rings and their excentric intersections presenting an extraordinary loveliness of colours.

Another result somewhat startled me. In some of the best glasses the movement of the Ross collar-adjustment for the position of the front lenses entirely decentred the solar disk, so that here two appeared occasionally instead of one.

This phenomenon compelled me to infer that in many cases the collar-adjustment may become a greater source of error than the "thickness" of glass cover which it is intended to compensate, and that therefore excellent glasses, constructed with a permanent setting, are preferable, especially as a new compensation can be effected, as described p. 435*.

The frequent appearance of several disks at once in the field of view caused me to suspect that the axes of the component lenses of the objectives were not always coincident.

An Andrew Ross "quarter," marked 1851, though of good quality, displayed several irregularly placed central disks, which formed so many different centres of diffraction-rings.

A Berlin glass of good quality showed a much finer primary black ring, and a splendid display of several coloured rings edged with black; in a deeper focus four false centres appeared.

It will not be out of place here to detail a few experiments conducted with the object of verifying the cause of irregularity in the primary black, and the particular signs of chromatic and spherical residuary aberration.

One of the finest "immersion" one-eighths of Powell and Lealand, made expressly for me in 1871, the aberrations of which were small compared with those of glasses of their old construction, was now used to form the miniature solar disk on the stage as derived from the distant prism-heliostat.

I then examined the solar disk with a Powell and Lealand one-sixteenth immersion-objective (1862 make) adapted to water in 1870, a water-film being introduced between the glasses, whose axes had been carefully adjusted to coincidence.

Two overlapping disks were now seen: each formed its own independent diffraction systems above the best focus, and vanished below it with a confused bright halo.

* By means of the compensating eyepiece.

In order to determine the cause of this and to insure one axis in the solar disk, I substituted for the miniature-giving objective a convexo-plane lens of half an inch focus; and as this gave too large a disk, the lens of the heliostat was reduced from three inches to an inch and a half focal length.

[Unless the solar disk is reduced, the splendid phenomena of the rings cannot be properly developed. Their number and colours change with the slightest change of the plane of focal vision; and a very fine and delicate focal adjustment-screw, as well as great firmness in the apparatus, are essential to a successful display of the rings in all their wonderful beauty and complexity.]

The disk formed by the simple lens was now scrutinized with the Powell and Lealand celebrated immersion "eighth." Deeper eyepieces and a lengthened tube were employed to subdue the intolerable brilliance of the coloured rings; they now exactly filled the whole field of view. At first, used dry (improperly), this objective displayed a crimson solar disk edged with an intensely black ring encircled with a much broader bright ring, resembling the planet Saturn viewed perpendicularly to the plane of his ring. (Pl. II. figs. 1-3.)

Deepening the focus with exceeding lightness of touch, the central disk now became pearly white, set off prettily by its companion black ring and a number of pale lavender, pale rose-colour, and then brilliant outer circles of bright green, with intervals of orange-red, and, more outwardly, circles of red merging into ill-defined black.

But as the glass was constructed for vision through a film of water upon a thin glass "cover," I now attached (by moisture) a small fragment of cover, 0.003 inch thick, to the eighth, and delicately focused down upon the solar spectrum.

The solar disk then appeared single, circular, and bounded by a clear sharp black edge almost perfectly circular.

Upon examining the axis in different planes of vision or different sections of the solar pencil, I counted no less than forty-eight magnificent rings (including the black rings and interspaces) displayed at one time in the same field of view. Derived directly from the sun, with the brilliance belonging to total internal reflection, this rich assemblage of gorgeous rings, rivalling each other in prismatic splendour, set off by the sharp contrasts of jet-black well-defined borders, and shaded with the most delicate tints melting into one another with an exquisite softness, reminded me of the eloquent and glowing language of the late Sir John Herschel when describing the phenomena of diffraction. Doubtless, however, these appearances surpassed in intensity and brilliance those he described. (Pl. II. figs. 2 & 3.) Careful measurements were made. The diameter of the central disk $\frac{1}{16}$; breadth of its black ring $\frac{1}{16}$.

My surprise was further increased by observing that, by lengthening the tube to increase power, I was enabled to cause each of the ruled

lines of a micrometric eyepiece (200 to an inch) to coincide exactly with the inner edge of each black ring; so that the breadth of each complete ring was exactly the same as that of the central disk, viz. $\frac{1}{18333}$ of an inch. (Pl. II. fig. 3.)

Slight changes in the colours of the rings were caused by the use of the Ross collar corrections.

In these researches a very near approximation to achromatism was signified by the whiteness of the central disk, the blackness of the fine rings contrasting finely with the intervening rings, which were then of a lavender-grey or very pale and yet brilliant lavender.

Destruction of spherical aberration appeared imminent when the rings, still coloured, were tolerably symmetrical on different sides of the finest focus with contrasting colours of the residuary spectrum. Mechanical errors were displayed by irregularity and complexity of form.

I shall now venture to give some particulars of the circular solar spectrum thus formed (as described) by a convexo-plane lens in a beam of sunshine examined by a high-quality immersion-objective armed with a small piece of broken cover attached by the cohesion of water.

*Description of the Rings of the Circular Spectrum of a
Convexo-plane Lens.*

Coloured rings.		Intervals.	
Solar disk	White.	Primary ring	Jet-black.
Ring II.	Pale lavender.	Secondary ring ..	Black.
" IV.	Lavender.	Third ring	"
" VI.	"	Fourth "	"
" VIII.	"	Fifth "	"
" X.	Pale rose.	Sixth "	Dark red.
" XII.	Bright green.	Seventh, "	"
" XIV.	"	Eighth "	"
" XVI.	"	Ninth "	"
" XVIII.	"	Tenth "	Black.
" XX.	Dark orange.	Eleventh ring	"
" XXII.	Deep "	Twelfth "	"
" XXIV.	" "		

Each of the rings, including the companion black or dark rings, appeared exactly of the same breadth, viz. 61 millionths of an inch, or nearly double the length of the wave of the extreme red ray; whilst the breadth of the primary black ring was nearly that of the wave-length for the line F in Fraunhöfer's spectrum, viz. 0.0004606 millimetre, or, since the French metre is in English inches

39.37078984,

it corresponds to 52256 waves to the English inch.

The delicate measurement of the primary black ring and disk was verified by a recording eyepiece micrometer. With this and the objective used it was found that one thousandth of an inch on the stage measured 1138 divisions, i. e. eleven turns of the divided head and $\frac{38}{100}$ of a turn. One division therefore represented

$$\frac{0.001}{1138} = \frac{1}{1,138,000} = \text{nine ten-millionths nearly.}$$

On estimating the breadth of the primary jet-black ring, and using a ruled glass micrometer, as I could detect no difference in the breadths of each ring, I felt justified in dividing the total diameter by the number of rings in order to obtain the breadth of one, which gave $\frac{1}{16333}$. A much deeper point in the axis showed a very deep blue central haze, paling outwardly, and then melting into a final red fringe.

The precision of the mechanical construction of this fine objective was thus revealed by the use of a simple convex lens of crown glass. Any deviation from accuracy was at once detected by the converging pencil of the plano-convex lens, consisting of shells of rays of various refrangibilities, having their several foci arranged along the axis. As this axis was necessarily single and unique, the interference phenomena, especially the sharpness and intensity of the jet-black rings, could only be so superbly exhibited by the best glasses. Inferior glasses blurred them and dulled the rich beauty of the colours.

Contrasting with this the performance of a variety of glasses, both English and foreign, very peculiar appearances arose which doubtless indicated grave errors of construction. I will venture briefly to mention some of these:—

(I.) A variety of spurious disks oddly arranged were displayed.

(II.) The beauty of the rings was entirely marred; and

(III.) Very few rings could be developed, and sometimes no black rings whatever (Pl. II. fig. 7, and Pl. III. fig. 14).

(IV.) Notched, grained, and spotted; the rings were sometimes irregular in shape (fig. 12).

(V.) A multitude of fine black excentric rings, evidently arising from different centres, were seen upon a leaden-grey field surrounding the central disk, the confusion of the rings causing a bad achromatism.

(VI.) An "engine-turned pattern" was not unfrequent, degenerating into a peculiar grained and mottled appearance.

(VII.) A majority of the glasses were overcorrected spherically.

(VIII.) Achromatism and aplanatism in our best adjustable glasses were found to be altogether incongruous.

As this result was alluded to at page 428, I proceed to relate the circumstances of the observation. I constantly found in pursuing these researches that either achromatism was sacrificed to aplanatism, or that the attainment of achromatism destroyed the brightness and truth of aplanatism. I may relate the following experience.

Whenever in watching the heliostat the sun was clouded over, as

alluded to at page 428, the microscopic miniature-perspective of the room and distant apparatus reappeared; and after various adjustments I obtained a perfect definition, free from mist and as clear and sharp as that of an opera-glass. The prism and lens of the heliostat then gave a pretty picture of the passing clouds, as well as the small details of the distant objects; but the instant the sun began to shine, before the rings dazzled the sight, every shining point appeared haloed with a corona of orange and red. I now turned aside the prism: then every polished point in the full sunshine exhibited the same halo. Again in the shade, the picture resumed its sharp definition. Waiting again for the sun, forth shone the orange haloes. The corrections were diligently plied till the halo nearly disappeared. The sun passed behind a cloud. To my astonishment the former sharp clear prospect was now *bedimmed with a general white mist, obscuring all the details before so beautifully clear*. The appearance of this white mist, *above the best focal point*, whenever achromatism was attained by varying the adjustments of the screw collars, now convinced me that the modern English glasses, when rendered achromatic, beget a residuary spherical aberration, obscuring delicate structures (such as I propose to describe further on) by a white mist, corresponding to spherical overcorrection, viz. the condition of the marginal rays for white light cutting the axis at points further from the centre of the lens than the central rays, and that until this fact is acknowledged an insuperable bar to the finest definition will continue to exist. (Pl. III. figs. 8 & 9.)

Dr. Colonel Woodward, U.S.A., having taken up the research, declares he found it impossible to photograph the most difficult beaded objects unless, upon examining their image on a white screen, he represented the beads red upon a blue ground; then, using a solution of the ammonio-sulphate of copper to absorb the red rays, and then only, could he photograph the results I had described * (M. Micr. Journ.).

Residuary spherical aberration, it thus appears, is the chief cause of

* In confirmation of the same principle the late Rev. J. B. Reade, F.R.S., wrote¹:—"Dr. Pigott has made also a very decided advance in the better correction of residuary aberration, a point which has, I believe, been almost completely ignored (nay even denied), until recently, by accurate observers as well as distinguished opticians. From my own experience in Dr. Pigott's studio, I have no doubt that his colour-test—a most interesting feature in his experiments—is the result of his finer balance of the aberrations . . . This new fact is one of the most striking phenomena in microscopical science of the present day. . . ."

"Whether this colour-test is explained on the theory of vibrating wave-length corresponding to the infinitesimal thicknesses of films . . . or upon their radiation, refraction, and internal reflection of the spherical beads of which all scales and diatoms appear to be built up, are questions so recondite as to be worthy of the consideration of the most advanced physicists of the day."

As residuary aberration is still denied, I may be permitted to quote 'The Student'

¹ Popular Science Review, p. 147, No. 35, 1870.

the difficulty experienced in defining organic particles—such as the molecules of physiological cells, blood-disks, mucous globules, and the discrimination of many forms of disease. It will probably remain uncorrected until opticians and observers abandon the false standards of definition still in vogue. If, then, it is at present impossible to avoid a residuary spherical aberration, whilst attaining very perfect achromatism, in the microscope, the finest definition will be obtained by stopping out the most obnoxious rays, either by using a monochromatic ray which suits the aplanatism, or using *bluish-green or blue glass* * to pale the red rays; for glasses may be aplanatic to one ray and not to another of a different refrangibility.

Before concluding this part of the paper I may be allowed to make a few practical conclusions for those who may wish to follow up this line of research :—

1. As stated in the paper “On a Searcher for Aplanatic Images,” regarding a convex lens as undercorrected, undercorrection is shown by the appearance of the rings below or beyond the focal point and evanishment into mist above it.

2. Similarity in the rings on both sides (with change of colour also) denotes a balance more or less delicate of the aberrations.

3. An excentric position of the solar disk and a crowding of the rings more closely on one side than the other of the circular spectrum denotes parallelism, but non-coincidence, of the axes of the convergent and divergent pencils (figs. 28 & 29).

4. Rare and beautiful forms, resembling parachutes, vases, or comets, made up of ellipsoid, parabolic, or hyperbolic diffraction-lines, denote obliquity (figs. 16–34).

5. Their form depends on the nature of the aberrations present, and the mode of arranging the axis of the cone of rays forming the solar disk.

6. Inaccurate centering of the component lenses, either at the heliostat or in the observing or miniature-making objectives, is shown by “excentric turning” patterns and the appearance of two or several central disks at the smallest focal spectrum.

7. The apparatus necessary to display these brilliant phenomena must be exceptionally heavy and steady, and the fine adjustment should have a screw 100 threads to the inch, as the ten thousandth of an inch in the axis of observation completely changes the aspect of the phenomena.

In none of these experiments did the supposed achromatism bear the severe ordeal of the circular solar spectrum. By no arrangements could

of February 1870. It states that the writer “has told very plainly two startling and unwelcome truths. First, that observers have not seen their favourite test-object properly; and, secondly, that their best object-glasses are afflicted with sufficient spherical aberration to render the structure which he describes invisible. . . and that all *difficult seeing* is in some suspense through these researches.”

* Appendix B.

colour be made to disappear. A white centre and exceedingly black rings, interspaced with a pale lavender and rose-colour, were the nearest approaches to perfect achromatism which I could produce.

On the Aberrations of Eyepieces, with suggestions for forming a Compensating Eyepiece for Microscopes and Telescopes, on the principle of searching the axis for Aplanatic Images.

The Astronomer Royal has given an account of a trial of several kinds of eyepieces and some of their bad effects*.

The use of a solar disk formed by an eyepiece fixed close to the prism-heliostat deprived of its lens, and examined by the method already described by means of accurately corrected objectives, places the achromatism of the eyepiece under a severe scrutiny. Very rich and beautiful colours are developed in the solar rings (previously obtained as pale as possible), corresponding to the extent of the chromatic errors. A Huyghenian eyepiece was placed close to the heliostat, so as to form a brilliant disk of the sun; the adjusted spectrum-apparatus immediately flashed with brilliant-coloured rings, before this appearing pale lavender and white.

During the use of the searcher for aplanatic images, it occurred to me to investigate the effects of pushing the eyepiece gradually nearer the object-glass without a searcher.

I discovered that, when within four inches, the definition showed violent undercorrection.

I now conceived the idea of substituting a traversing movement of the eyepiece, especially for glasses unprovided with a Ross collar, as a correction for thickness of cover.

A very firm sliding-tube was constructed; and I now found I had substituted a range of several inches, as a correction, for that of a few hundredths of an inch used in the Ross adjustment.

Experiment.—Adjusting the apparatus and the screw collars of the objectives for severe testing, a bunch of small glass drops, of diameter 0.04 inch, was suspended in front of the heliostat, so as to present a minute image of the sun. The searching eyepiece being placed at 10 inches distance from the stage of the microscope in the plane of which the solar disk is formed, and the minute solar disk observed in a state of balanced corrections, it was found that as the eyepiece was traversed towards the disk it became gradually more and more undercorrected.

It now became evident that this movement was, upon a large scale, equivalent to the effect of the Ross collar movement upon a minute scale.

It now occurred to me, as a thick cover and a water-film overcorrected

* Astronomical Notices.

an objective, that a dry objective might in many cases be transformed into an "immersion" simply by advancing the eyepiece; also that a sufficient variation of interval between the front lenses might in many cases enable a dry lens to act as an immersion.

[The immersion principle is valuable for the increased volume of the cone of rays radiant from the illuminated particle mounted in balsam, a much larger pencil reaching the objective *vis-à-vis* water than can possibly be effected *vis-à-vis* air, the "critical angles" of total internal reflection which determine the form of the caustic being so much larger in passing from glass into water than into air. I have shown elsewhere that the volume of the cone of rays transmitted from a radiant particle placed in balsam and surmounted with a thin glass cover is about four times greater *vis-à-vis* water than *vis-à-vis* air; that result is explanatory of the greater brightness of the immersion lens.]

The question of the spherical and chromatic aberrations of eyepieces has occupied the attention of the most distinguished mathematicians, and may theoretically be considered nearly exhausted; yet the practical detection of its existence is known to few, as it is liable to be mixed up with the objective aberration. The methods described are equally applicable to eyepieces as to objectives. (Appendix B.)

The construction of a compound compensating eyepiece which should be almost perfectly free from this residuum next engaged my attention.

From the discovery that the advance of an eyepiece towards the objective caused a violent *undercorrection in the refocused objective*, it became evident that a *shortened* microscope could be employed as a compound eyepiece nearly free from the usual aberrations, provided its object-glass were properly *overcorrected*, as compared with its performance at the usual standard distance of ten inches.

The new eyepiece is finally corrected on the circular solar spectrum (herein described), being regarded and treated as a real microscope. Its object-glass, considering the exceedingly small pencil engaging it, may conveniently be formed of slightly overcorrected achromatic lenses, compensated by a variable interval. I have found an inch focal length sufficiently deep, mounted with a low eyepiece. The substitution of this compensating eyepiece for the ordinary deep Huyghenians afforded that degree of comfort in observation corresponding to enlarged pencils.

After adjustment it is quite as applicable to examine the performance of telescopes as microscopes. The adjustment is thus accomplished:—

1. The instrument, mounted as a complete microscope, was adjusted for the most perfect definition on an uncovered object; and supposing the glasses A, B (adjustable by a variable interval) defined perfectly with the usual length of tube 10 inches, they require overcorrection* or separation for a shorter tube of 6 inches.

* Considering the small angular aperture, a single set of achromatic lenses might be

2. Various eyepieces were inserted, and the lenses A, B separated or closed more or less till the most perfect definition was attained.

3. Now, regarding the instrument as a perfectly corrected compensating eyepiece, it is transferred to the tube of another microscope, the objective of which is again adjusted by the screw-collar for the most distinct definition.

Tested on the principles glanced at in this paper, the corrected compensating eyepiece, free from the usual aberrations, may be confidently employed to test the circular solar spectrum of a minute distant solar heliostat-disk, formed in its focal plane, by the objective also of a proposed telescope.

On new Adjustments for the Object-glasses of Telescopes and compensations for residuary variations.

Applied to telescopes the compensating eyepiece is adjusted somewhat differently.

The instrument having been severely tested for an uncovered object, and used for this purpose as a compound microscope (its front glasses A, B being considerably overcorrected when tried for ten inches), the telescope is directed to the solar disk formed at as great a distance as is attainable by a prism-heliostat; the condition of the diffraction-rings will then display the state of the telescopic objective.

If several eyepieces and adjustable lenses are applied in succession, under precautions (such as securing a sufficiently small and distant solar disk), the aberrations of the telescope can be accurately observed*.

A careful consideration of the compensations effected in the microscope,

(1) by separating the objective lenses,

(2) by separating the eyepiece lenses,

(3) by traversing the eyepiece in search of aplanatic images,

leads to the conclusion that, provided the front lenses of the compensating eyepiece be separable more or less, and the distance between them and the eyepiece be somewhat variable, then a new correction for the telescope is given by also separating the component lenses of its object-glass by minute intervals.

To accomplish this accurately, sliding telescopic tubes, one carrying

constructed and employed; but their correction is much more tediously attained than by separable sets of lenses.

* The eyepiece recommended to thus test the solar disk formed by a telescope is to be used precisely upon the same principles as a microscope; the greater the aperture of the telescope the greater will be the power applicable, *i. e.* by deeper eyepieces and objectives, which must have much more widely separable lenses than usual for "cover correction," as regards screw-collar adjustments, if employed in order to compensate for shortness of body. The aperture employed by this eyepiece in all cases equals the angular aperture of the object-glass of the telescope at the focus of the eye-lens.

the flint glass and another the crown, are furnished with long rods terminating with similar screws with divided heads.

If on testing the telescope severely by applying a microscope accurately adjusted to the solar disk, diminished sufficiently by a distant inverted objective in front of a heliostat described, the telescope object-glasses be overcorrected, the separable lenses of the telescopic object-glass will require closing together slightly by moving the screw-heads through equal spaces; and, *vice versâ*, slight residuary aberrations will be also affected by changes in the compensating eyepiece. The tubes should be ground (in the manner of making levels) truly to preserve the centering of the objective lenses.

Another form of correction is the concave achromatic lens, now called the Barlow lens. If this lens be formed of an overcorrected combination, its effect on the final aberration will vary as it receives a larger or smaller pencil, *i. e.* as its position between the eyepiece and objective of a telescope or microscope is made to vary. The traversing principle here advocated has therefore several important advantages worthy of investigation, and may possibly form a correction for changeable refracting powers of the atmosphere.

In the compensating eyepiece a large visual pencil is substituted for a minute one and the same ease and comfort is obtained with it as if the linear aperture of the telescope were greatly enlarged. An ocular of one-inch focus may be used instead of another ten times as deep.

Slight changes in the over- or undercorrection of the telescope depending on the changeable density of the atmosphere may also probably be compensated by over- or undercorrecting this eyepiece.

Moreover the special aberrations of the eye-glasses may be ameliorated by a slight change of form and an alteration in the interval between them.

NOTE.—June 25, 1873.

The scarcity of sunshine this year has prevented me carrying out a complete series of experiments in telescopes. My excellent 8½-inch Browning equatorial (one of his very best) shows little spherical aberration. Two very good achromatic telescopes with the ordinary eyepieces gave imperfect jet-black rings, few in number, with the ordinary Huyghenians; but with the compensated eyepiece I counted six intensely black rings. The definition was excellent.

I trust observers with more convenient appliances and range of prospect will be induced to try a series of experiments on correcting residuary aberration in their telescopic object-glasses by the methods here laid down, and the writer will at any time be honoured by exhibiting to them some of the phenomena here described, as already done to Dr. Gladstone, F.R.S., and Mr. Beaumont, F.R.S.

APPENDIX.

A.—Definition of minute organic particles.

A new fiftieth immersion lens, price thirty guineas, has been made for me by Messrs. Powell and Lealand; with this glass, without any obliquity, and using a tube four inches shorter than usual and a B eyepiece, the upper continuous ribs of the *Podura* were resolved into strings of blue sapphire-like beads appearing perfectly circular. The interspaces between the markings at a lower focus showed lower strings of white beads.

Monads appeared blue, swimming about in the water used, and also lying in well-defined masses; some of them could be seen to rotate; the cilia were invisible, but the movements gave strong indications of them. —April 21, 1873.

A blue glass improved the definition. Coal-oil lamp and $1\frac{1}{2}$ -inch open condenser object-glass.

At present nothing is more difficult of definition in the microscope than an assemblage of minute refracting organic particles. Virtually forming disks of light, these display the diffraction-errors and phenomena more or less vividly. No English microscopist has yet succeeded, so far as is known to the writer, in displaying, in the apparently blank spaces, the beaded structure seen between the celebrated exclamation-markings of the *Podura* test-object. Here a closely packed mass of organic particles, highly refractive and transparent, obscure each other; brilliant points are swelled out exceedingly, for a theoretical solar disk one millionth of an inch* in diameter appeared as large as a disk the sixty thousandth. To define accurately bright organic particles, such as those of the smallest test-*Podura*, the molecules of cancer-cells and other diseased forms, and monads (minute atoms of metal by reflected light might also be named), is at present impossible; when such delicate forms are in quest, all rays of an aberrating character must necessarily be extinguished.

Illustration.—Examining with an excellent new $\frac{1}{50}$ immersion at sunset, as already alluded to, it was only when a peculiar bluish-green of the sky was thrown by an open excellent $1\frac{1}{2}$ -inch objective glass condenser upon the stage that the internal organic particles of a delicate *Podura*-scale became visible. Doubtless in this case the diffraction-spectra here had reached a minimum†. In the details given in the accompanying note several points are particularly worthy of notice—the use of an intermediate lens (amplifier), the sudden appearance of organic

* Easily formed by placing a minute lens before the heliostat.

† Dr. J. J. Woodward, U.S. Army Museum, has published a remarkable confirmation of this point. The existence of intercostal beading, first discovered by the writer in 1862 and published in May 1869, has been (and still is) disputed by English observers in no amicable spirit. He states:—

“Shortly after my paper had been forwarded I received a visit from Mr. Joseph

particles, monochromatic light, and the view of a higher focal plane than that which showed the markings.

B.—Eyepieces.

The spherical aberration of positive eyepieces may be examined as follows :—

The positive in an inverted position is placed under the stage, and forms an image of the solar disk for examination.

DESCRIPTION OF THE PLATES.

Circular Solar Spectrum.

PLATE II.

Fig. 1. Primary rings finely defined at the first visible focal plane.

Fig. 2. Secondary rings, at a deeper focus. The central disk should have been squared off.

Fig. 3. Exhibition of the greatest display of rings, each annulus having its own breadth equal to that of the central disk, which is a brilliant white, the succeeding lavender, rose-colour, and red rings separated by dark rings, the first few of which are jet-black.

Figs. 4, 5, 6, 7. Development of two disks instead of one; also of four irregular disks showing the existence of displaced centres and irregular diffractions.

PLATE III.

Figs. 8, 9. The miniature prospect of the distant window is displayed sharply in fig. 8. So soon as the sun began to shine, the blazing prism being quenched by turning it aside, every brilliant point became irradiated with an orange-red halo

Beek, who is spending some months in the United States. He exhibited to me (with his fine high-power objectives) several slides of the scale of the *Lepidocyrtus curvicolis* [test-scale *Podura*] collected by himself from carefully identified insects. He aimed at the exhibition of the exclamation-marks, as such, using a right-angled prism, achromatic condenser, and small coal-oil lamp. Before leaving Washington he was so good as to give me one of these slides, indicating on it a scale which appeared particularly adapted to the display of the exclamation-marks. Certainly this scale, which was $\frac{1}{16}$ of an inch in length and $\frac{1}{32}$ broad, surpassed in the distinctness of its markings any of the scales of this species which have hitherto come into my possession. With central *monochromatic light*, the immersion $\frac{1}{4}$, and *amplifier*, I obtained a negative showing the exclamation marks better than any representation of the kind I have yet been able to obtain. I send herewith a paper print. The magnifying-power is 3200 diameters." (Dr. Woodward also favoured the writer with copies.)

"But immediately afterwards, with the same optical combination and magnifying-power, without any change in the cover correction, by simply rendering the illuminating pencil oblique, and *slightly withdrawing the objective from its first focal position*, I obtained a negative which displays the "head-like" or varicose appearance of the ribbing more satisfactorily than I had previously been able to do." (Month. Micr. Journ., June 1871, p. 245.) [The writer has *italicized* the quotation.]

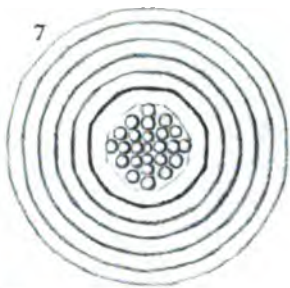
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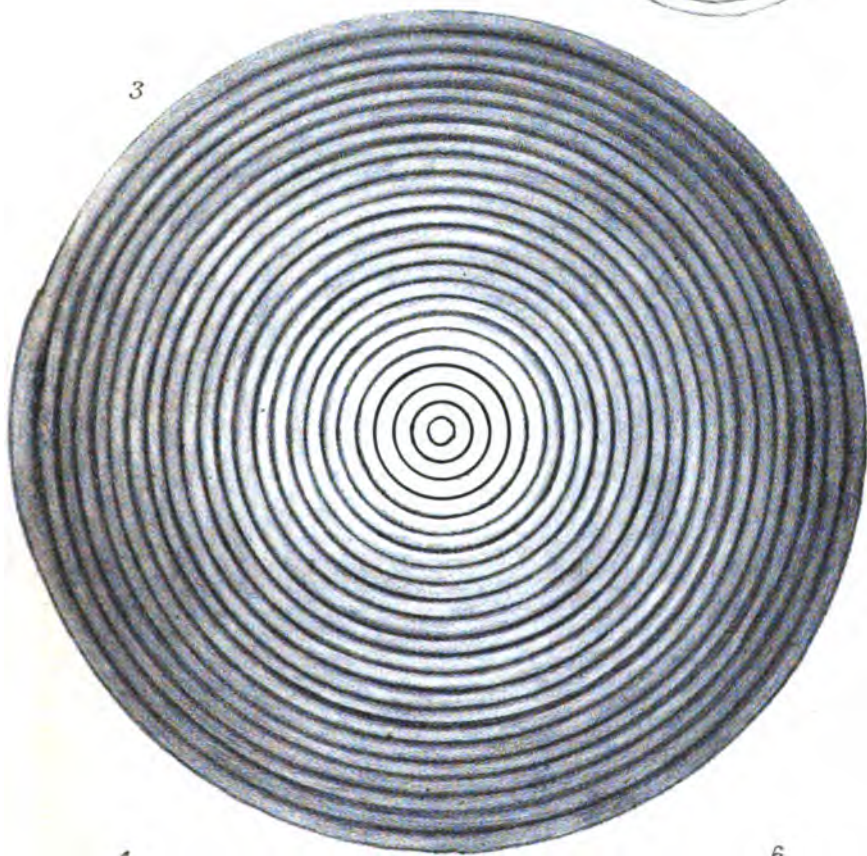
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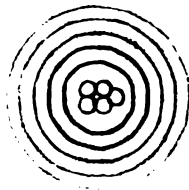
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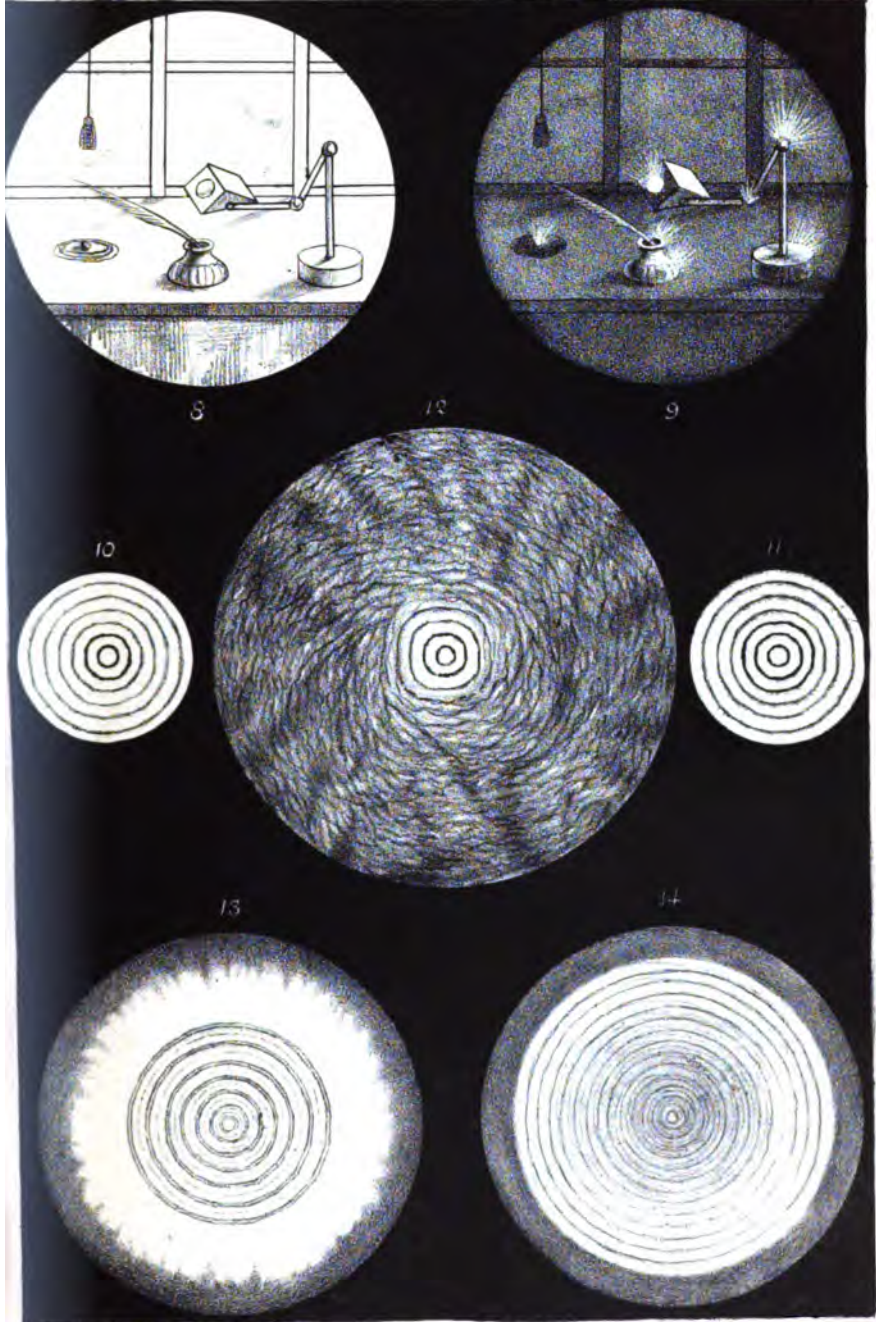


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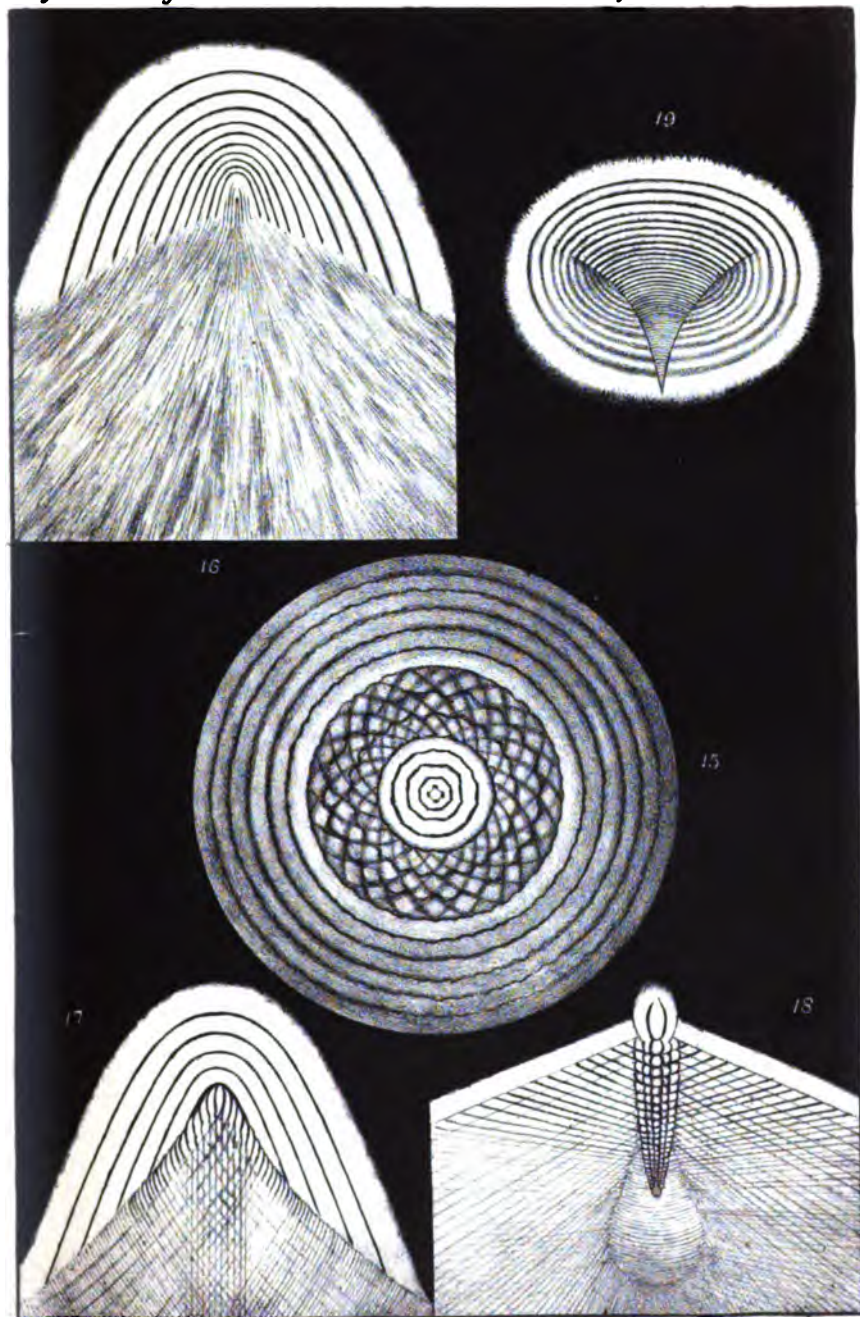
A.T. Hollick del. et lith.

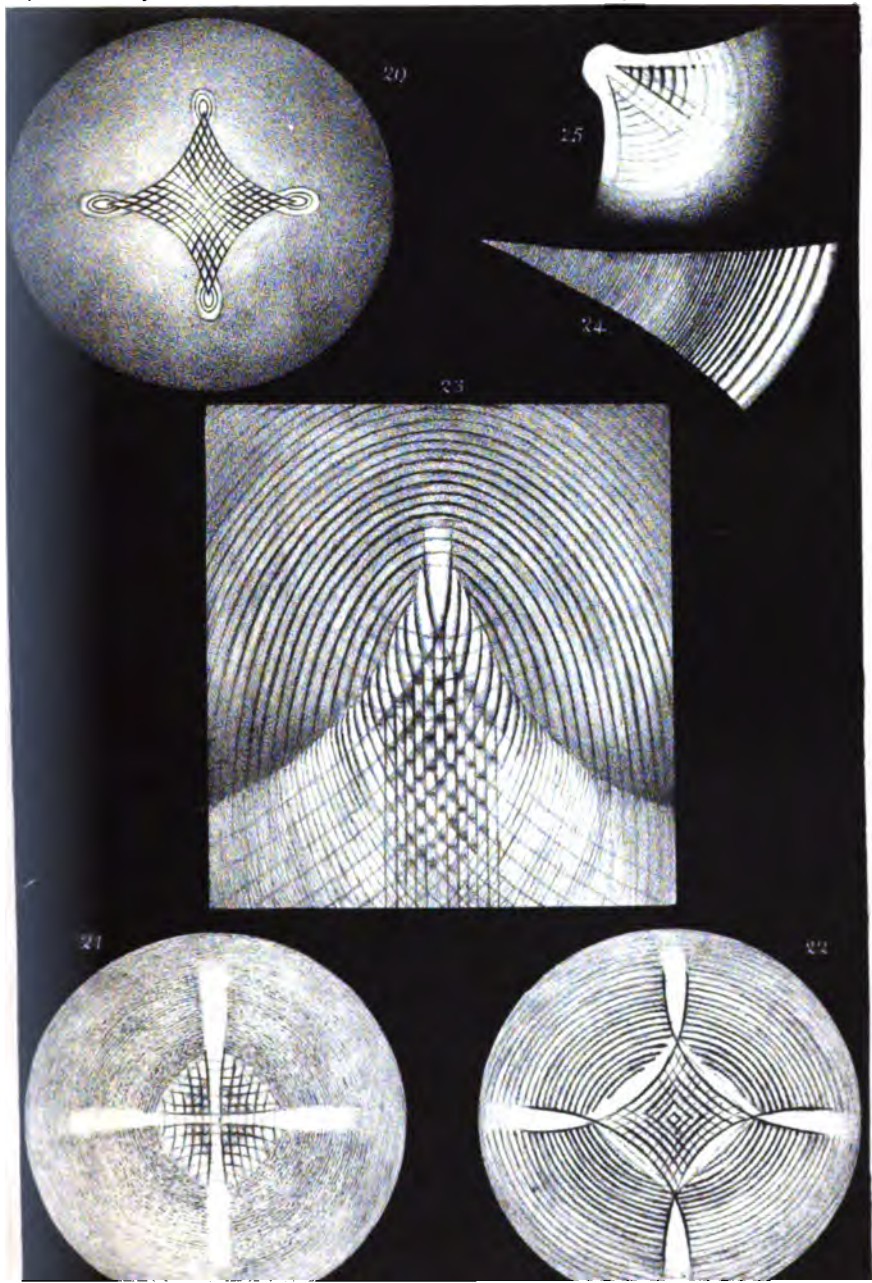
W. West & Co. lith.

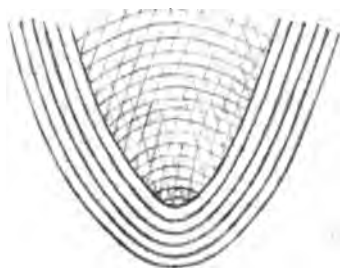
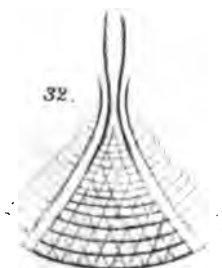
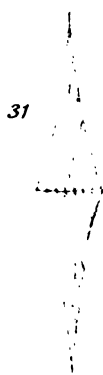
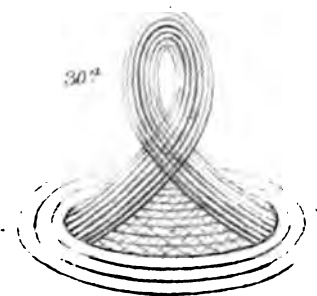
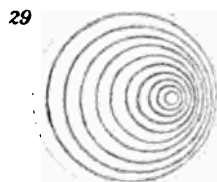
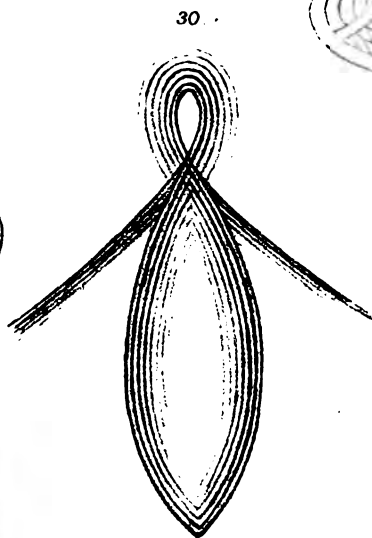
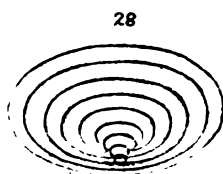
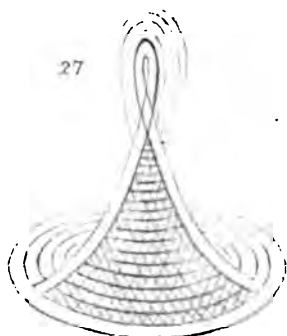
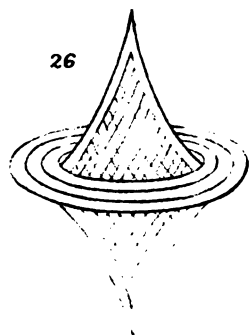


A.T. Hollidge del. et lith.

W. West & Co. imp.







(fig. 9). If the colour were corrected by change of the general adjustments so as to destroy halo, then the prospect in fig. 8 became enveloped in a strong white mist of uncorrected residuary aberration.

Figs. 10, 11. The slight deviations from the true circular form, owing to imperfect glasses, are here well represented.

Fig. 12 displays the very delicate engine-turned pattern and obscuration of the diffraction-rings by a badly constructed glass (cheap German).

Fig. 13 shows a delicate set of rings between the coarser, expanding by a change of focus in a different manner. The approaching halo and fog are well delineated.

Fig. 14 shows the blurred appearance of the rings when the spherical aberration is excessive; similar also to the evanishing spectrum occurring before the fog appears.

PLATE IV.

Fig. 15. In this case the heliostat was placed nearly forty feet distant. The internal lenses of a fine $\frac{1}{2}$ objective being all removed, the thick front only was employed to form the miniature on the stage. A peculiar irregularity in the central jet-black rings is supplemented by extraordinary excentric lines bordered by a new order of peripheral rings, obeying a different law of expansion. Viewed under Powell's best dry $\frac{1}{2}$, a half-inch single plano-convex lens being used as eyepiece.

The next figures illustrate the effects of obliquity, the previous drawings exhibiting various effects during coincidence of the axis of the microscope with that of the miniature-forming lens.

Figs. 16 & 17 represent a very beautiful variety of hyperbolic diffraction-lines seen when the axis of the solar ray is inclined about 6 degrees to that of the microscope. Distance of heliostat 20 feet, magnifying-power 1000. Powell's best $\frac{1}{2}$ forms the miniature observed with best dry eighth.

Fig. 17. Miniature objective, a $\frac{1}{10}$ Gundlach immersion used dry. Microscope objective best $\frac{1}{2}$ dry eye-lens, plano half-inch, parabolic curves and fine diffractions, obliquity 5 degrees.

Fig. 18. The appearance within the focus of the best and first resolution of the fog of undercorrection.

Fig. 19. Slight obliquity and undercorrection.

PLATE V.

Figs. 20, 21, 22. Best $\frac{1}{2}$ and $\frac{1}{2}$ plano-concave stage-lens.

The circular spectra here delineated are produced by slight obliquity, and represent the appearances at different focal planes. The colours are extremely brilliant, and the lines perfectly sharp in their tracery.

Fig. 23 shows the lines formed by the circular solar spectrum viewed with the greatest obliquity attainable, the elliptic lines representing a plane cutting both side of the cone of converging rays.

Figs. 24 & 25 introduce a new order of figures formed by placing a mercurial globule 10 inches from the stage, and placing the $1\frac{1}{4}$ -inch objective condenser with its axis considerably inclined (an angle of 15°) to that of the microscope; the images of the solar disk then took these and the forms in figs. 26-34, as seen with a microscope armed with a good eyepiece (Kellner 1-inch) and a fine $\frac{1}{2}$ objective.

PLATE VI.

This Plate gives various very beautiful transformations of the circular solar spectra as seen by viewing the solar disk of the mercurial globule portrayed obliquely on the stage, which vary in their forms according as the glasses are under- or overcorrected, as depicted in the figures, which it is needless here to describe in detail.

Fig. 34 is another form of mercurial globule, beautifully defined.

The figures displayed by the magnified artificial star for oblique reflection render it probable that the obliquely illuminated mercury globule, viewed directly in close proximity to the front glass of the microscope upon the stage, is a very imperfect test; and the methods here described are submitted as possessing very superior delicacy and convenience.

XXII. "On Comparative Vegetable Chromatology." By H. C. SORBY, F.R.S. &c. Received June 9, 1873.

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Introduction.

The study of colouring-matters as described in the following paper requires the use of a somewhat special kind of chemistry and of spectro-

scopic instruments designed for the purpose, but yet it cannot be looked upon as being either chemistry or spectrum-analysis. It is also requisite to make use of light as a reagent, and yet the subject could not be called photography or even photo-chemistry; and though most intimately connected with biology, it could scarcely be regarded as a branch of that science. It appears to me most desirable to adopt some name for the whole subject, including whatever modifications of other branches of science are requisite for its efficient study, and none would meet the requirements of the case better than *Chromatology*. I trust that the facts I am about to describe will be sufficient to show that we have such a wide branch of science before us that it deserves some special name. Then, again, since I now purpose to describe the points of agreement and difference in the colouring-matters of various classes of plants, it appears to me that no better title could be adopted than the one I have chosen, viz. *Comparative Vegetable Chromatology*.

My attention has been for a long time directed to the colouring-matters of plants, but it is only within the last year that I have discovered the means of carrying out the inquiry in a satisfactory manner, and convinced myself that the most valuable conclusions are to be learned, not by looking for the rarer or more attractive substances which give remarkable spectra, but from the qualitative and quantitative determination of the different coloured constituents of complicated mixtures, some of which substances have no striking properties, and might easily be overlooked; though perhaps of great importance in connexion with the life of particular plants. The successful carrying out of this inquiry will necessarily be the work of years; for not only is the whole field comparatively unexplored, but it will be requisite to examine all classes of plants and many of animals, at various seasons of the year and when growing in different natural and artificial circumstances, and also to study the relation of the different colouring-matters to one another, and the action on them of light and chemical reagents. In every department of the subject very interesting questions remain to be answered; and the following paper must be looked upon merely as a general outline of what I have hitherto been able to learn, and as an indication of what may be expected to result from further research.

The Absorption-band-raising Power of Solvents.

The identification of the individual colouring-matters has been effected by means of their spectra, sometimes as seen by examining the plants themselves, but generally when dissolved in various liquids, with or without the addition of appropriate reagents; but any other peculiarities were taken advantage of, according to the circumstances of the case. For further particulars connected with the general method I refer to my former papers*.

* Proc. Roy. Soc. 1867, vol. xv. p. 433. Quart. Journ. of Microscopical Science,

Here, however, I would remark that the exact position of the absorption-bands given by particular substances varies considerably, according to the nature of the solvent, even though it is quite neutral and exerts no chemical action on the colouring-matter. Thus, for example, the bands may lie much nearer to the red end when dissolved in bisulphide of carbon than when dissolved in benzole or absolute alcohol. This kind of fact has been often noticed by others, but its bearing on some important questions has generally been overlooked. Apparently, as a rule, the bands are more raised towards the blue when the colouring-matter is dissolved than when in a solid state, and this power of raising them varies with the solvent; but the extent also depends very much on each particular substance. I have carefully determined the order in which various liquids thus raise bands, and compared it with the order of their specific gravity, and of their refractive and dispersive power, but have hitherto failed to recognize any simple connexion between what may be called the *band-raising power* and any other physical property. In some cases the position of the bands given by the solid substance appears to vary according as it is in a colloid or a crystalline state. Though at first this difference in position may seem to be an objection in the use of the spectrum method, yet it only shows the need of comparing together our specimens in exactly the same condition, and it may be taken advantage of as a means for distinguishing closely allied substances which may be influenced in a very different manner and for enabling us to ascertain the condition of a colouring-matter as it exists in an animal or plant. It is thus possible to determine whether it is in a free state or dissolved in an oil or wax, and in the latter case whether it is liquid or solid. In this manner I have been able to detect a difference in the relative quantity of oil in different parts of the same leaf. Various forms of spectrum-microscopes were employed, according to the requirements of the case; and the amount of material at command would often have been too small for examination with any other kind of instrument; so that I think I may fairly say that comparative vegetable chromatology is the first great subject which has resulted from the use of the spectrum-microscope.

Separation of Colouring-matters by chemical means.

On the present occasion I propose to consider almost exclusively those colouring-matters which are soluble in bisulphide of carbon and in fixed oils, but insoluble in water. These appear to be by far the most important in connexion with plant life, or at all events are more uniformly distributed. Those soluble in water and insoluble in bisulphide of

carbon or fixed oils are much more numerous, and often very partially and, as it were, accidentally present: those of the former class may often be separated from one another by means of bisulphide of carbon, alcohol, and water, mixed in proper proportions. As an illustration I take the very simple case of olive *Alga*, like *Fucus* or *Laminaria*. They should be well crushed, slightly dried, and heated in spirits of wine of the usual strength. Absolute alcohol must not be used, since it dissolves out the chlorophyll in such a manner that it is not carried down as usual, when agitated with bisulphide of carbon. When cold the solution should be agitated in a test-tube with so much excess of the bisulphide that a considerable quantity subsides to the bottom, carrying with it the whole of the orange xanthophyll* and the greater part of the blue chlorophyll, leaving some of the latter in the alcohol along with nearly all the fucoxanthine and chlorofucine. This solution should then be removed by a pipette, and agitated with a fresh quantity of bisulphide, the process being repeated until it subsides with only a very slight tinge of green. The alcoholic solution, on being evaporated and treated with bisulphide, yields approximately pure fucoxanthine. Uniting the different lots of bisulphide containing chlorophyll, and agitating them over and over again with fresh spirit, taking care that a small excess of bisulphide is always present, the whole of the chlorophyll may be removed, and the orange xanthophyll left in the final residue. On adding a little water to the washings containing the chlorophyll, the dissolved bisulphide is precipitated, carrying down all the chlorophyll. By proceeding in a similar manner, with or without the addition of weak acids or alkalies, various other colouring-matters found in plants may be more or less perfectly separated. All these processes should be carried on without exposure to strong daylight, or else some of the constituents might be in great measure destroyed.

Photochemical Analysis.

There are many cases in which certain colouring-matters cannot be separated in a satisfactory manner by merely chemical methods, and then fortunately we are able to resort to what I propose to call *photochemical analysis*. The action of light on the coloured constituents of plants was made the subject of an excellent memoir by Herschel, published in the *Philosophical Transactions* for 1842, p. 181; but the object he had in view and the methods he employed were altogether different to mine. He adopted no means for separating the different constituents of the plants, and some of his solutions probably contained as many as ten different coloured substances; and, moreover, at that time no attention had been paid to their spectra. He exposed paper coloured with such mixtures to the solar spectrum, so as to learn the effect of each portion; and his results are of great interest, as showing that the decomposition

* The exact use of all these terms will be explained in the sequel.

is due to the luminous rays, which act with variable intensity in different parts of the spectrum, according to the nature of the substance. On the contrary, my method of photochemical analysis consists in using either the whole or particular rays of the sunlight passing through coloured glasses as reagents to decompose some of the constituents of a mixed solution, and leave others in a state of approximate purity. In employing light for such a practical purpose, it would be almost impossible to use a spectrum. Without this method the study of the colouring-matters of plants could not be carried out in a successful manner. I have sometimes studied a substance for several weeks before I was able to prove that it was a mixture by ordinary chemical methods, whereas I afterwards found that this could be easily proved by simply exposing the solution to the sun. I, however, now give only such examples as have an important bearing on the subject before me. For instance, in the case of some *Algæ* it is impossible to separate the xanthophyll from the orange xanthophyll, and the spectrum of the mixed solution in bisulphide of carbon gives absorption-bands in an intermediate position, not agreeing with those characteristic of either one or the other. It might thus be imagined that these *Algæ* differed entirely from allied species, in containing a single and peculiar kind of xanthophyll instead of the usual two, thus making an apparent complete break in what is really perfect continuity; but by exposing the solution to the open sun the orange xanthophyll is so much more rapidly destroyed than the xanthophyll, that in a while the absorption-bands of the latter are seen in their normal position; and knowing this fact, and comparing the spectrum with that of the original, it is easy to see that at first there must have been a mixture of the two kinds. Of course in all such experiments care must be used to prevent overexposure, and the specimens should be examined from time to time. In most cases it is also extremely difficult to separate yellow xanthophyll from lichnoxanthine; but by exposure to the sun this xanthophyll is soon decomposed, its absorption-bands disappear, and the uniform absorption of the lichnoxanthine remains. Solutions which originally give very different spectra can thus be proved to be merely variable mixtures of two or more substances, each well known in a pure state. There are, however, cases in which both constituents would be destroyed by exposure to white light; but one is changed chiefly by one kind of rays, and the other by another. For example, when the mixed solution in bisulphide of carbon of phycoxanthine and orange lichnoxanthine, obtained from certain *Algæ* and lichens, is exposed to the open sun, the phycoxanthine is soon destroyed, but at the same time some of the orange lichnoxanthine, and it is almost impossible to cease exposing at the proper point; when kept under deep green glass, however, it is easy to decompose nearly all the phycoxanthine without materially diminishing the amount of the orange lichnoxanthine. Other

applications of this method will be given when describing special cases.

General Action of Light.

Independent of its value in such practical analyses, the study of the action of light on vegetable colouring-matters is of great interest in connexion with changes that occur in the plants themselves; but since so much still remains to be learned, I will now only give a general summary of what I have already ascertained. As a general rule, when solutions of such substances are exposed to the open sun, their colour is destroyed without the formation of any intermediate coloured product; but in a few cases entirely different coloured compounds are generated, and afterwards themselves destroyed on further exposure. This bleaching effect depends upon the combined presence of light and air; for if kept in the dark, or if sealed up in tubes quite free from air, and then exposed to the sun, very little or no such alteration occurs. Other things being equal, the rate of change varies enormously according to the nature of the substance, and, with the same substance, varies much according to the solvent.

Sensitizers.

The presence of minute quantities of certain oils very greatly increases the rate of change in a solution exposed to the light, and it is convenient to call this their *sensitizing* action. Probably in many cases it depends upon their ozonizing influence. Such substances occur in certain plants, and the result is that the effect of an equal amount of light is sometimes increased threefold or even more. If this substance could be procured pure in moderate quantity it would be a most valuable reagent, since it has very little or no effect on colouring-matters unless exposed to the light. In the mean time I have found turpentine a very useful sensitizer, the chief objection being that it sometimes acts as an oxidizer, even in the dark, and is too energetic, so that very little should be used. Oil of citronelle has the most powerful reverse action of any that I have examined, and greatly reduces the effect of light. I therefore often use it to protect from change.

Correlation of Optical and Chemical Characters.

We might naturally expect that those rays of light which are absorbed most strongly would be most instrumental in effecting decomposition. If this be a law, it is subject to modifying conditions, not yet apparent; and since many of the changes do not take place without the presence of air, they are due not to light itself alone, but to oxidization intensified by its presence. At all events it is not merely the blue end of the spectrum (the so called chemical rays) that act, for some substances are rapidly destroyed in red light, as was shown by Herschel and subsequently by Gerland*. I have met with so many cases where a number of substances

* Archives Néerlandaises, 1872, vol. vii. p. 1 *et seq.*

having many properties in common, but differing in the position of their absorption-bands, are decomposed when exposed to the light in the order in which the bands lie towards the red end, that I cannot but think there is some connexion between these facts. When the spectra are quite unlike (for example when one has two and the other three bands, or two at a different interval, or no bands at all) there is no such connexion. Those which seem to approach most closely and uniformly to the law belong to simple groups, and give spectra more or less closely analogous, as it were, to the same musical chord in different keys; and this leads me to think it probable that, if we had such a perfect series of substances, the rate at which they would be decomposed by the action of light would vary directly as the length of the waves absorbed, or as would express the facts equally well in many cases, as though the power of decomposition varied directly as the width of the spectrum over which the absorption extends, *i.e.* as the number of different rays absorbed. This would also, in great measure, correspond to what occurs when the same substance is dissolved in liquids which cause the absorption to extend more and more towards the red end. Whether this be a general law or only a common fact, the results are of very great importance in connexion with my present subject. The effect of this difference in the rate of change will be better understood by means of the following rough illustration. Suppose that there is a mixture of three analogous colouring-matters, *a*, *b*, and *c*, the absorption-bands of *a* being nearest to the red and those of *c* to the blue end of the spectrum, and suppose that on exposure to the sun, in the time required to destroy one part of *c*, two parts of *b* and three of *a* are decomposed, it is obvious that the original proportion between the different substances would soon be very materially changed. This would alter the ratio in which they would be decomposed by further action until a sort of equilibrium was established; so that, whatever might have been the original proportion, there would at length be a sort of ultimate vanishing ratio, depending on the nature of the substance and of the solvent and on the kind of light and its intensity. I find this fluxional manner of viewing such facts often very useful, and cannot but think that it might be introduced with advantage into similar subjects. However, for the sake of simplicity, supposing that the assumed rate of decomposition remained constant, and adopting arbitrary units, which only express the ratios for each substance, we should have some such result as the following:—

Original state	$10a + 10b + 10c$
Successive states, after more and more exposure to the sun.	$\left\{ \begin{array}{l} 7a + 8b + 9c \\ 4a + 6b + 8c \\ a + 4b + 7c \\ 2b + 6c \\ 5c \end{array} \right.$

If instead of an analogous substance (*a*) we had an entirely different

(A), very slightly affected by light, the result might be expressed thus:—

Original state	$10A + 10b + 10c$
Subsequent states	$\left\{ \begin{array}{l} 9A \\ 8A \end{array} \right. + 5c$

Comparative Quantitative Analysis.

By applying the above-described general methods to the study of the mixed coloured solutions obtained from plants, there is usually no serious difficulty in carrying out a *qualitative* analysis, even though, as often happens, half a dozen or more coloured substances are present. It is, however, often very desirable to determine them *quantitatively*; not, indeed, their relative *weights* in one case, but the relative amount of each particular colouring-matter in different plants or in the same when grown in different conditions—a kind of *comparative quantitative* analysis. In some cases this can be done without difficulty. The various coloured constituents can be separated with sufficient accuracy, and the relative volumes of equally strong solution determined. The most convenient plan is to have test-tubes of the same internal diameter, and to dilute the solution in one or in both until the intensity of the colour is exactly the same, or, still better, until the characteristic absorption is equal, as seen in the spectroscope, with equal illumination. The relative length of the column in the tubes then, of course, gives the relative amount of the particular colouring-matter. We may also thus ascertain the relative quantity of two or even more in the same solution, by making the respective bands equal, first in the case of one and then in that of the other. I intend to construct a form of spectrum-microscope and prepare apparatus specially suited for accurate comparisons, which require perfectly equal illumination; but even those analyses which I have been able to make by means of my present instruments are sufficient to show the value of such quantitative methods. I have hitherto chiefly compared the different specimens together, but for the future I shall prepare suitable standard solutions, sealed up in tubes with as little air as possible, protected by oil of citronelle; and if it turn out that they remain unaltered by long keeping, it will thus be possible to determine the relative amount of the various colouring-matters in plants growing at different seasons of the year.

Fundamental Colouring-matters.

Trusting that the above account will be sufficient to indicate the general characters of the methods I have employed, I now proceed to the consideration of the individual colouring-matters. The total number met with in plants is very great, possibly some hundreds; but a large proportion occur only in the petals, fruits, seeds, and roots. The study of these will no doubt throw much light on particular questions, but

they are of far less importance in connexion with my present subject than those which occur in the leaves of the higher or in the fronds of the lower classes of plants. I purpose, therefore, to confine my remarks almost exclusively to these. Even then it is very necessary to distinguish between those which appear to be essential for the healthy growth of each class of plants and those which have the character of accidental products, formed under particular conditions, and often absent, without apparently interfering with the normal development of the plant. There is exactly the same difference in the case of animals. The hæmoglobin of the blood and the coloured substances in bile are of the utmost physiological importance, whereas those in the hair and feathers are either of no importance whatever, or are of advantage to the animal only in some very indirect manner. Similarly, the colouring-matters belonging to the chlorophyll and xanthophyll groups appear to be essential, whereas those of the erythrophyll group have all the characters of products varying with accidental circumstances; and if they are of any value to the plant, it is only indirectly, as, for instance, in attracting to the petals insects instrumental in causing fertilization. These accidental and unessential colouring-matters are often so much more conspicuous than those of real importance, that they might easily lead any one to conclude that the study of the colouring of plants could not lead to satisfactory results, since very often the colour is not of value even in distinguishing different species. It is, however, not the mere *colour* that is of importance, but the nature and relative amount of what may be called the *fundamental colouring-matters*, of which there are altogether somewhere between a dozen and a score. For example, we often have varieties of plants which have red leaves instead of green: this is owing to the unusual amount of erythrophyll; and, independent of this, the fundamental constituents of the higher classes of plants are present in their normal quantity, or their equilibrium only slightly modified by a corresponding difference in conditions. In those classes of plants where conspicuous accidental colouring-matters are seldom developed (as, for instance, in the different groups of marine *Algæ*), the importance of the colour is universally admitted; and I find that when studied in accordance with the principles described in this paper, the presence or absence of the different kinds of colouring-matters, or their relative amount in different classes of plants, is in some way or other connected with very important differences in organization and vital energy.

Description of the Groups of Colouring-matters.

In accordance with these principles, I now propose to describe those colouring-matters which appear to be of the greatest physiological importance, dividing them into various groups, and in the case of each group arranging them in the order of their optical characters—i.e. commencing with those in which the absorption extends nearest to the red

end of the spectrum, and following the order in which it retreats towards the blue. This is, moreover, the order in which they are decomposed by light, and also often that in which they vary in other important peculiarities. I have chosen specific names which indicate the generic relations of the separate substances.

Stokes's Researches.

In the Proceedings of the Royal Society for 1864* there is a paper by Professor Stokes, rather more than a page long, on the supposed identity of biliverdin with chlorophyll, with remarks on the constitution of chlorophyll, which, in my opinion, in the space of a few sentences contains more valuable information on that subject than all other subsequent papers put together. In this he says, "I find the chlorophyll of land-plants to be a mixture of four substances, two green and two yellow, all possessing highly distinctive optical properties. The green substances yield solutions exhibiting a strong red fluorescence; the yellow substances do not. . . . Green sea-weeds agree with land-plants, except as to the relative proportions of the substances present; but in olive-coloured sea-weeds the second green substance is replaced by a third green substance, and the first yellow substance by a third yellow substance, to the presence of which the dull colour of those plants is due." No particulars were given respecting the manner in which these different colouring-matters had been separated, nor any description of their individual characteristics, and this is probably the reason why these statements have had so little influence on subsequent researches. If I may judge from my own experience, an experimenter might well have paid a great amount of attention to the subject without being able to discover the nature of the evidence which had led the author to adopt such conclusions; and it would require a very considerable amount of independent discovery to enable any one to appreciate their real merit. As for myself, I investigated the subject quite independently, and it was not until after I had satisfied myself as to the real facts of the case that I compared my results with those described by Stokes in the above-named paper, and then saw that they corresponded in many of the most important particulars, though I have no doubt that I was led to them by studying a very different class of facts. The principal green substance of the author is manifestly my blue chlorophyll; his second green substance my yellow chlorophyll, and his third green substance my chlorofucine, whilst his third yellow substance, found in olive sea-weeds, must be my fucoxanthine. His other two yellow substances must, in some way or other, represent the four yellow substances described by me as orange xanthophyll, xanthophyll, yellow xanthophyll, and lichnoxanthine, with perhaps a little of my orange lichnoxanthine. It will thus be seen that my conclusions fully confirm what he has said respecting the compound

* Vol. xiii. p. 144.

nature of chlorophyll (I mean of the green constituent of leaves, independent of the yellow colouring-matters), though, as far as I am aware, no one else has hitherto adopted that opinion. Having said this much, I now proceed to give an account of my own experiments, and of the conclusions which seem to explain the facts in a satisfactory manner.

Chlorophyll Group.

This comprises three perfectly distinct substances, soluble in bisulphide of carbon and insoluble in water, which agree in having absorption at both ends of the spectrum, in having well-marked absorption-bands at the red end, to the principal of which the strong red fluorescence is related, and in being decomposed with greater or less facility by weak acids into new products, giving somewhat analogous, but yet quite distinct spectra to those characteristic of them in their natural state. All are rapidly decomposed in strong sunlight, when air is also present, but not otherwise.

Blue Chlorophyll.

I was first led to distinguish the compound nature of the chlorophyll in ordinary green leaves by studying that found in *Oscillatoria*, as named in former papers. This kind is that which occurs throughout the whole vegetable world, and the difference consists in the usual but not universal presence of another along with it, which second kind differs in the highest and in the very lowest classes of plants. Blue chlorophyll can be best obtained in an approximately pure state from olive *Algae*, like *Fucus* or *Laminaria*, by the method already described. Its spectrum shows three absorption-bands at the red end, that nearest the extreme red being by far the most intense. Unless the solution be very strong, the whole of the green and a considerable portion of the blue are transmitted, so that the colour is such a blue-green that the term *blue chlorophyll* appears to be very appropriate. Any decided band in the green is proof of the presence of a small quantity of the well-known product of the action of the acids, which very readily decompose it. This change may be avoided by crushing up acid plants with a little bicarbonate of ammonia before dissolving the chlorophyll; but it is far better to make use of those whose juice is more neutral. Mere boiling in water, as adopted by some experimenters, is not sufficient, since the alteration will take place even when the chlorophyll is not in solution. The absorption at the blue end is due to a broad band between the blue and the violet, which can be seen only in a moderately pure solution, with excellent daylight. Besides the product of the action of acids just alluded to, several others can be prepared from blue chlorophyll by decomposing it with acids or alkalies; but they are of little interest in connexion with living plants, except as showing that it is in a state of very unstable equilibrium.

Yellow Chlorophyll.

This substance has not been obtained in a pure state, but yet suffi-

ciently so to enable me to determine many of its peculiarities. The best method of obtaining it is to boil *Uva latissima* first in water and then in spirit. If this plant cannot be procured, the small green *Algæ* so common on damp wood and stones may be used with advantage, and need not be boiled in water. The deep green solution in alcohol should be agitated with bisulphide of carbon, and a few drops of water added, if requisite, to cause nearly all the green colouring-matter to be carried down, and nearly all the xanthophylls to remain in solution. On removing this and agitating the bisulphide with fresh spirit of wine of the usual strength, taking care to have a considerable excess of the bisulphide present, a large portion of the yellow chlorophyll and some of the blue are dissolved. Removing this solution to another tube, adding a little water, and separating the diluted alcohol, the precipitated bisulphide should be evaporated to dryness at a gentle heat. On redissolving in benzole and examining the spectrum of a properly diluted solution, it will be seen that it differs in many important particulars from that of pure blue chlorophyll. Besides bands exactly corresponding with those characteristic of that substance, it has an independent dark and narrow band in the red, and another much broader in the middle of the blue, as though the solution were a mixture of blue chlorophyll with some other substance*—a conclusion completely borne out by further examination, since the same blue kind as that found in olive *Algæ* occurs almost free from this second substance in the residue left after repeated agitation with spirit. Taking, then, a solution in benzole of blue chlorophyll, of such a strength that the principal absorption-band in the red may exactly correspond with that seen in the above-named mixture, and comparing together the spectra side by side, it is easy to see what absorption-bands are due to the other substance. Again, since blue chlorophyll is far more easily decomposed by weak acids than the yellow, and yields a product very slowly decomposed by light, we may take a mixed solution in which the blue has undergone this change, and, by treating it in the manner described above, may obtain a mixture of yellow chlorophyll and the product of the action of acids on the blue. Dissolving this in benzole, and taking a solution of the product of the action of acids on the pure blue, of such a strength as to give the corresponding absorption-bands equal when seen side by side, those due to the yellow chlorophyll can easily be distinguished; and on exposing for a while to the direct rays of the sun, these bands disappear, whilst those due to the product of the action of acids on the blue kind remain just as in the specimen used for comparison. By these means I have been able to prove that the spectrum of yellow chlorophyll dissolved in benzole has a very dark and well-defined narrow band in the red, further from the red end than that due to blue chlorophyll, and probably another much fainter in the orange. There is also a broad and strong

* See my paper "On Mixed Colouring-matters," Monthly Microscopical Journal, vol. vi. p. 124.

band in the middle of the blue ; the whole of the blue is absorbed, and not, as in the case of blue chlorophyll, a considerable portion transmitted. The general colour is therefore such a yellow-green that the term *yellow chlorophyll* seems very appropriate. When dissolved in alcohol the principal band in the red is made about twice as broad, and half as dark, as when in benzole or bisulphide of carbon, and can no longer be seen quite detached and separate from that due to blue chlorophyll, especially if the relative amount of the latter be considerable, and merely causes an unsymmetrical shading on the side towards the orange. It is to this circumstance in great measure that must be attributed the fact of the compound nature of chlorophyll having been overlooked, alcohol having been so generally used as the solvent.

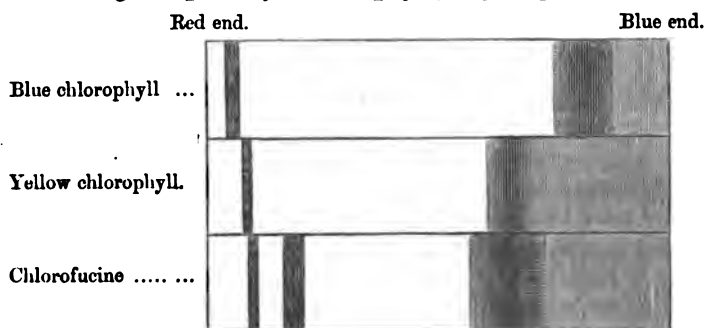
Chlorofucine.

It is some time since I was first led to believe that a third kind of colouring-matter belonging to the chlorophyll group existed in some freshwater *Algæ*. I first obtained it separate from blue chlorophyll in examining a complicated mixture of that and other substances obtained by Mr. Charles Horner from an *Actinia*, viz. *Anthea cereus*, var. *smaragdina*. The special bands due to this other substance had attracted his attention, and led him to think that the chlorophyll, as a whole, was different to that in plants. I have since found that this third member of the group is one of the fundamental constituents of *Fucus* and other olive *Algæ*, and have therefore named it *chlorofucine*. In order to obtain it approximately pure, the fresh fronds should be crushed in a mortar and slightly dried, so that on adding ordinary spirit of wine too much water may not be present. For the same reason the solution should be kept covered up at a gentle heat, and not boiled. On agitating with repeated quantities of bisulphide of carbon, the whole of the blue chlorophyll may be removed, and the alcoholic solution then contains much fucoxanthine and the chlorofucine, showing two well-marked absorption-bands, one between the red and the orange and the other in the yellow. On adding water and agitating with more bisulphide, both these substances are carried down ; and on evaporating the mixed solution, the chlorofucine is often partially lost by decomposition in some unknown way. To prepare it free from fucoxanthine, the above-named alcoholic solution should be diluted with an equal bulk of water, a drop or two of ammonia added, and agitated over and over again with fresh bisulphide, until it subsides to the bottom almost colourless. By thus adding ammonia the chlorofucine is retained in solution, and the whole of the fucoxanthine is carried down in the bisulphide. The dilute alcoholic solution is then of a pale green colour, and contains chlorofucine mixed with a yellow substance soluble in water. On evaporating to small bulk at a gentle heat, the loss of alcohol causes impure chlorofucine to be deposited ; and after transferring to a narrow tube, allowing it to subside, and removing the yellow aqueous solution, almost pure chlorofucine

can be dissolved out from the deposit by means of absolute alcohol: at all events it is free from chlorophyll, and perhaps only contains a small quantity of the pale yellow substance. The colour of this solution is a yellow-green, very much like that of a crude solution from green leaves. The spectrum shows two dark absorption-bands, one between the red and the orange, and another between the orange and the yellow. The whole of the green is transmitted, but the whole of the blue cut off; and when the solution is dilute, a broad and somewhat obscure band may be seen in the centre of the blue, if excellent daylight is used. The addition of a little ammonia slightly increases the intensity of the band nearest the red end. When thus modified by dilute alkalies, chlorofucine is very sparingly, if at all, soluble in bisulphide of carbon. Excess of hydrochloric acid causes immediate decomposition into a new compound, with a single absorption-band in the orange. Weak acids slowly produce the same effect; and it is important to bear this in mind, since this product of the action of acids, and not the original substance, is met with in studying certain plants.

The difference between the spectra of the above-named three green substances will be better understood by means of the following figure, which represents the absorption-bands as seen in solutions diluted so as to show those at the blue end, and only the darkest and most characteristic of those in the red.

Fig. 1. *Spectra of the chlorophyll group compared.*



Fluorescence of the Chlorophyll Group.

The position of the narrow bright red bands seen in the spectra of the light of fluorescence of these three members of the chlorophyll group differs even more than that of the absorption-bands, since in the case of chlorofucine the bright orange-red band almost coincides with the dark band of absorption, whereas in the case of both blue and yellow chlorophyll the more red bands lie just on the red side of the centre of their absorption-bands. The result is that the three different narrow red bands characteristic of the light of fluorescence of the three different

substances have their centres nearly at equal intervals. All three kinds are so rapidly decomposed by strong direct sunlight, that the study of their fluorescence is a matter of some difficulty. Whilst thus alluding to this subject, I would take the opportunity of describing a method which I have found extremely convenient in deciding whether a substance possesses genuine fluorescence, or whether it is spurious and due to the presence of solid particles so minute that the solution may appear quite clear and transparent. I illuminate by means of light which has passed through a solution of sulphate of didymium. If, then, there be only a spurious fluorescence, the spectrum shows all the absorption-bands of the didymium—in fact we obtain merely a spectrum of that body modified by reflection; whereas if it be a true and pure fluorescence, no trace of the bands of didymium can be seen. If there be a genuine fluorescence along with some reflected light, a more or less faint spectrum of didymium will be visible due to this spurious light, with a brighter part due to the genuine fluorescence.

Action of Acids on the Chlorophyll Group.

As previously mentioned, all the above-named members of the chlorophyll group are decomposed with greater or less facility by acids. Each gives rise to an independent product; so that, though they have many generic peculiarities in common, they must be regarded as well-marked species, and not in any way as mere accidental modifications of a single substance.

Xanthophyll Group.

A considerable number of different colouring-matters might be classed with this group, but on the present occasion I purpose to consider only the six most commonly met with in leaves or fronds or in fungi. They are characterized by being insoluble in water, but soluble in bisulphide of carbon and fixed oils, and by giving spectra with two more or less well-marked absorption-bands, varying in position with the particular substance, and very considerably raised towards the blue end when a liquid of great band-raising power is used as a solvent. When the solution is strong, the whole of the blue end is absorbed up to these bands. Sometimes three absorption-bands may be seen; but in that case it is generally easy to prove, either by chemical or photochemical methods, that the third band is due to a second substance. Weak acids or alkalies have no immediate effect in altering the position of the bands; but when dissolved in absolute alcohol all the members of the group are slowly decomposed by a little hydrochloric acid, gradually fading to a colourless solution, even in the dark. The rate at which this takes place and the production of intermediate coloured products depend to some small extent on the presence of air, but still more on the presence of minute quantities of sensitizing substances. The nature of this action is still

rather obscure; but a further study of it seems likely to throw light on the cause of the production of some of the colouring-matters found in particular plants. When such a sensitizing substance is absent, so that the alteration takes place too slowly, a little turpentine is an excellent substitute. The changes which occur vary according to the particular species of the group, and thus furnish a valuable means for distinguishing them, or for recognizing small quantities of some when mixed with others.

Phycoxanthine.

This name was first proposed by Kraus* for a substance he obtained from *Oscillatoria*; but there is no doubt whatever that his preparation was a mixture of two or three distinct kinds of colouring-matters, which can easily be separated, and do occur separately in other plants. That for which I adopt the name may be obtained in the most pure state from the lichen *Peltigera canina* when growing in a very damp and shady place, or from some species of *Oscillatoria* when growing quite open to the sun. The various coloured substances should be dissolved out by hot spirit, and the solution when cold diluted, if requisite, with a little water, and agitated over and over again with fresh quantities of bisulphide of carbon until it subsides tinged only a pale pink. By this means almost the whole of the other colouring-matters are carried down, with the exception of the lichnoxanthines. On evaporating the alcoholic solution and redissolving in bisulphide of carbon, a red solution is obtained, which, when more dilute, is a fine pink, and gives a spectrum with two excellent absorption-bands in the green. When it is dissolved in absolute alcohol these bands lie much further from the red end, and the colour is sufficiently yellow to justify the name *phycoxanthine*, or, at all events, to make it undesirable to introduce another. On adding a little hydrochloric acid and turpentine, the colour gradually fades, without the production of any well-marked intermediate coloured substance. I am much inclined to believe that this phycoxanthine may be formed from orange xanthophyll, both artificially and naturally; but since this change occurs only in very rare and special cases, it probably depends on the presence of some sensitizing substance, only occasionally present or under conditions not yet understood.

Peziza Xanthine.

This is the name I propose for an orange-yellow colouring-matter found in *Peziza aurantia* and some other fungi. It resembles phycoxanthine in many particulars, but differs in the position of the absorption-bands, which lie further from the red end.

Orange Xanthophyll.

This is one of the most universally distributed of all vegetable colour-

* Chlorophyllfarbstoffe, p. 109.

ing-matters, since it occurs in greater or less quantity in all classes of plants, including fungi. It may be obtained in its most pure state from the orange-coloured antherozoids of *Fucus serratus* in spring, which exude from the antheridia when the fronds are kept damp. These may be washed off the fronds into water and separated by filtering, after any coarser particles have subsided. The colouring-matters should then be dissolved out in hot strong alcohol, and the clear solution agitated with bisulphide of carbon. This carries down the orange xanthophyll, which may be somewhat further purified by repeated agitation with fresh alcohol. It may also be obtained from *Peltigera canina* or from *Oscillatoria* grown in bright light by a similar process; but in that case, in order to separate the chlorophyll, it is requisite to agitate very many times with fresh spirit and repeated additions of bisulphide of carbon, so as always to leave only a small quantity undissolved. The success of this method depends upon the fact that, unless the relative amount of the orange xanthophyll be too great, the whole is carried down in the bisulphide of carbon, when agitated with spirit of the usual strength. The amount present in the leaves of the higher classes of plants is smaller than in those just named; but by following the same process a final residue of orange-colour may be obtained, which, however, is seldom free from impurities. When pure and dissolved in bisulphide of carbon, the colour is a slightly pink-orange, and the spectrum shows two obscure absorption-bands in the blue end of the green and green end of the blue. These lie much further from the red end when it is dissolved in absolute alcohol; and on adding a little hydrochloric acid and turpentine it gradually becomes colourless, sometimes turning slightly green; but this is probably due to the presence of another substance—generally to a trace of yellow xanthophyll.

Xanthophyll.

I propose to restrict this name, as applied to an individual substance, to the principal constituent of the mixture which has usually been so called. This generally, if not always, must have been mixed with my yellow xanthophyll and lichnoxanthine. Xanthophyll, as the term is applied by me, may be obtained in the most pure state from *Porphyra vulgaris*, by heating in spirit, removing the blue chlorophyll by agitation with bisulphide of carbon, and then adding water to the alcoholic solution, which precipitates the bisulphide along with the xanthophyll. The solution in bisulphide of carbon is of a slightly orange-yellow colour, and gives a spectrum with two absorption-bands between the green and the blue. When dissolved in absolute alcohol they lie at the green end of the blue; and on adding a little hydrochloric acid and turpentine the colour gradually disappears, without the production of any new intermediate coloured substance, or only such a faint tinge of green as is probably due to impurity. Xanthophyll may also be procured from dark-coloured varieties of Wallflowers (*Cheiranthus cheiri*), and with more

difficulty from green or yellow leaves ; but being then mixed with yellow xanthophyll, it is almost impossible to obtain it pure, and it therefore turns green when treated with hydrochloric acid and turpentine. If no sensitizer be present, such mixtures may change so slowly that the blue product may fade as fast as it is formed, and never be very visible.

Yellow Xanthophyll.

This may be obtained in its most pure state from different kinds of bright yellow flowers, as, for instance, the yellow *Chrysanthemum*. When dissolved in bisulphide of carbon it is of a clear lemon-yellow colour, differing most decidedly from that of xanthophyll properly so called. The spectrum shows two very distinct absorption-bands in the green end of the blue: these lie still further within the blue when absolute alcohol is used as the solvent, and, on adding a little hydrochloric acid, the yellow xanthophyll is decomposed, first into a new and paler yellow substance*, with the absorption-bands at a different interval and more towards the blue end, and afterwards into a fine deep blue colouring-matter, which will be more fully described further on. Yellow xanthophyll may be obtained in an impure state from ordinary green leaves, by first removing the chlorophyll and then adding successive small quantities of water, agitating with bisulphide of carbon after each addition, and keeping separate each quantity thus carried down. The first contains much xanthophyll and a little yellow xanthophyll, and each subsequent quantity contains less of one and more of the other, as may be proved by both chemical and photochemical methods, or by the position of the absorption-bands, until, towards the last, the yellow xanthophyll greatly preponderates, mixed, however, with so much lichnoxanthine that the absorption-bands seen in its spectrum are far less distinct than when it is prepared from the petals of such flowers as I have named. On treating the solution in absolute alcohol with hydrochloric acid, though it turns to a blue-green, a residual yellow colour (lichnoxanthine) remains when the blue has faded, which is not the case when pure. If the leaves have an acid juice, the last residue consists, not of yellow xanthophyll, but of the above-named yellow product of the action upon it of weak acids.

General relations of the Xanthophyll Group.

These five different kinds of colouring-matters belonging to the xanthophyll group are related to one another in a very interesting manner. Making use of same scale of measurement and notation as described in my former paper †, the centres of the absorption-bands of

fact has also been noticed by Stokes (Journal of Chem. Soc. vol. ii. 1864,

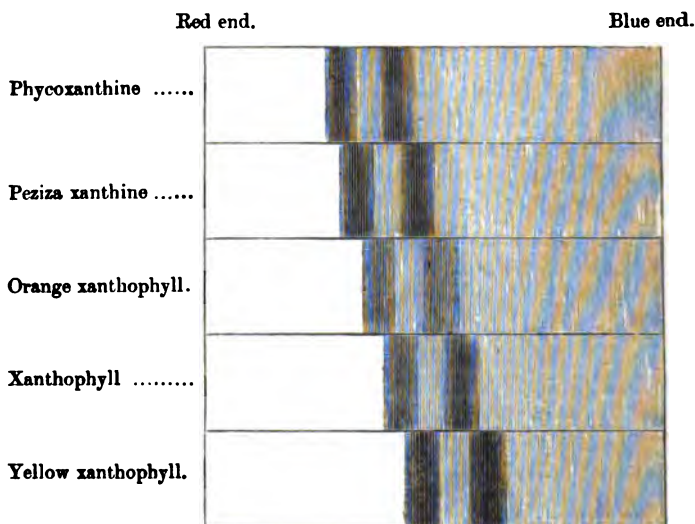
of Roy. Soc. 1867, vol. xv. p. 434.

these various substances, when dissolved in bisulphide of carbon, are very closely as follows ($D=3\frac{1}{2}$, $F=7\frac{1}{2}$):—

Phycoxanthine.....	5	$6\frac{1}{2}$	
Peziza xanthine	$5\frac{1}{2}$	7	
Orange xanthophyll	6	$7\frac{1}{2}$	
Xanthophyll		$6\frac{1}{2}$	8
Yellow xanthophyll.....		7	$8\frac{1}{2}$

These relations will be better understood by means of the following figure:—

Fig. 2. *Spectra of the xanthophyll group compared.*



When these various substances are dissolved in benzole, their absorption-bands are all equally raised towards the blue end, so that we appear to have a remarkable series of very closely related substances.

Action of Light on the Xanthophyll Group.

The behaviour of these substances when exposed to light is very interesting. Taking a mixture of any two in bisulphide of carbon, and exposing to the open sun, that which has its absorption extending over the wider space, and has its absorption-bands the nearer to the red end, is destroyed more rapidly than the one of which the absorption extends over a less space, and the absorption-bands lie nearer to the blue end. If any contiguous two species be mixed, the rate at which they are decomposed differs so little that very much of both must be destroyed before the one whose absorption lies the nearer to the blue end is left

almost free from the other. On the contrary, when we have a mixture of two more extreme, a very considerable quantity of one is left when the other has disappeared.

I fear it will be difficult to find any other such series of very similar colouring-matters; but if, on further investigation, such a connexion between the rate of decomposition and the absorption of light turn out to be a general law, it will be an additional proof of some kind of connexion between chemical and optical characters. The most striking modification of this rule that I have so far met with, is in the case of phycoxanthine and orange xanthophyll when mixed with chlorophyll in solution. When alone, dissolved in a mixture of alcohol and bisulphide of carbon, the phycoxanthine is so much more rapidly decomposed by bright sunlight than the orange xanthophyll that the relative amount of the latter is soon increased to double the original, as determined by separating them from one another; whereas if a considerable quantity of blue chlorophyll be present, the very opposite result occurs, as proved by comparative analysis. Here, then, we have a most striking effect of the presence of the chlorophyll, which itself was more rapidly decomposed than either of the other substances mixed with it. Having only very recently established this fact, I have not yet been able to decide whether it is due to the loss of the energy in decomposing the chlorophyll of those rays of light which, in its absence, would have been instrumental in decomposing the phycoxanthine, or whether it depends upon some chemical action connected with the presence of the changing chlorophyll; but it seems to me that we have in such a case the first example of a new branch of inquiry, which can hardly fail to be of value in explaining the influence of chlorophyll in the growth of plants. Many facts seem to prove that one substance may protect another by absorbing the active rays or by consuming the oxygen, which, in its absence, would decompose the other.

Fucoxanthine.

This is the name I propose for the principal colouring-matter of *Fuci* and other olive *Algæ*. It may be obtained in the manner already described, only that in order to separate it from the chlorofucine, after separation of all the chlorophyll, a few drops of ammonia should be added to the alcoholic solution, and the whole diluted with an equal bulk of water. The bisulphide of carbon is then precipitated with almost all the fucoxanthine, whilst nearly the whole of the chlorofucine remains in the dilute alcohol. As thus purified, fucoxanthine dissolved in bisulphide of carbon is of a beautiful amber colour, and its spectrum shows two obscure absorption-bands, the position being intermediate between those of orange xanthophyll and xanthophyll, so that a mixture of these gives nearly the same spectrum. The difference, however, is completely proved by other facts. The bands of fucoxanthine are much less raised by alcohol and other liquids of high band-raising power than the bands of

those two kinds of xanthophyll, and it resists the action of light far more than they do. A mixture of pure orange xanthophyll and xanthophyll in absolute alcohol treated with hydrochloric acid would, at the most, give only a pale green, whereas fucoxanthine is changed into a splendid blue substance, and subsequently into a sort of claret-coloured, before finally and slowly fading. Though the spectra of fucoxanthine and yellow xanthophyll are essentially different, yet this blue product is the same. It absorbs the whole of the red end of the spectrum, not transmitting even the extreme red; and on adding an excess of ammonia this absorption is entirely removed, and the colour is changed to a bright yellow. The spectrum then shows a well-marked absorption-band at the violet end of the blue. On adding excess of hydrochloric acid, the original blue colour is restored; and hence this substance has the unusual peculiarity of being made blue by acids and yellow by alkalies. Hitherto I have never met with it in plants themselves. Taking every thing into consideration, we must look upon fucoxanthine as closely related to xanthophyll; but at the same time the different effect of solvents in raising the absorption-bands, and the greater permanence when exposed to light, may perhaps make it desirable to class it in a subgroup. The dull olive colour of those *Algæ* in which it occurs so abundantly (the *Melanospermæ*) is apparently mainly due to it in a *free state*, not dissolved in any oil. On comparing the spectrum of the light transmitted by a frond in its natural condition with that of the light transmitted by a portion which has been boiled for a short time in water until the colour has changed to green, it may be seen that the absorption due to the fucoxanthine is considerably raised, just as if at that high temperature it were attacked and dissolved by the oil present in the plant.

Lichnoxanthine Group.

This is a name which I propose for a number of colouring-matters found in a great variety of plants. Two are more especially abundant in fungi; but since they occur in a more marked manner in lichens than in any other plants having true leaves or fronds, it appears to me desirable to adopt the name *lichnoxanthine* rather than *fungoxanthine*, which perhaps may be found convenient for some of the substances found more especially in fungi. So far I have met with three different kinds of lichnoxanthine in the leaves and fronds of plants. They are all insoluble in water, and soluble in bisulphide of carbon, but have a far greater tendency to dissolve in alcohol; and in some cases easily pass into a modification which is insoluble in the bisulphide, and yet readily dissolved by absolute alcohol. The spectra do not show any well-marked absorption-bands, but merely a general absorption, extending more or less from the blue end, according to the particular species, and very little if at all modified by the addition of weak alkalies or acids. All are of a much more permanent character than the different kinds of xanthophyll,

resisting the action of light and acids so well that they are very often obtained as residues when all the colouring-matters of that group have been decomposed. They, however, do ultimately fade. It is not yet certain whether these substances are essential for the growth even of lichens, or only constant products.

Orange Lichnoxanthine.

This particular species occurs in many fungi, but I have more especially studied it as obtained from lichens. *Peltigera canina*, growing well exposed to the sun, should be crushed and heated in spirit. After separating the whole of the chlorophyll in the manner already described, the alcoholic solution should be evaporated to dryness, and the lichnoxanthine and the phycoxanthine dissolved out by bisulphide of carbon. On exposing this to the sun under green glass until the pink tinge is lost and the absorption-bands of the phycoxanthine have disappeared, the lichnoxanthine remains of an orange colour. Its spectrum, when the solution is sufficiently strong, shows absorption of the whole of the blue and green, gradually shading off to about the end of the yellow. The modification soluble only in alcohol may be obtained from *Platysma glaucum* and many other lichens by agitating the alcoholic solution with bisulphide of carbon, and then diluting the alcohol with water, so that all the substances soluble in the bisulphide may be carried down in it, all soluble in water remain dissolved, and the modified orange lichnoxanthine may be deposited in a free state, and, after separation, dissolved in absolute alcohol.

Lichnoxanthine.

This is the most common member of the group, and is so widely distributed that it occurs in almost all plants, though sometimes in very small quantity and not easily detected. It is best obtained from the yellow fungus, *Clavaria fusiformis*. In the absence of that plant, it may be prepared from the root of the carrot by dissolving out the colour in spirit, and agitating with bisulphide of carbon to remove the orange xanthophyll and the greater part of the xanthophyll. On evaporating the alcoholic solution to dryness bisulphide of carbon dissolves the lichnoxanthine along with a good deal of xanthophyll and yellow xanthophyll; but by exposing the solution to the sun, those two colouring-matters may be destroyed, and the lichnoxanthine left alone. When pure and dissolved in bisulphide of carbon it is of fine, somewhat orange-yellow colour, and the spectrum shows a very uniform absorption, extending over the whole of the blue, and ending suddenly about the centre of the green. It is so difficult to separate this lichnoxanthine, properly so called, from orange lichnoxanthine, that I shall frequently call the mixture the *lichnoxanthines*, without attempting to give them alone.

Yellow Lichnoxanthine.

I have adopted this name for a substance very similar to that just described, but giving a spectrum in which the absorption does not extend so far from the blue end, so that the colour is a more pure yellow. It is best obtained from *Parmelia (Physcia) parietina* by the same process as that just described.

Phycocyan Group.

I have described the two principal species of this group in former papers*, and will therefore only briefly allude to a few characters. *Phycocyan* gives a spectrum with a well-marked absorption-band in the orange, and has a very intense red fluorescence, due to a narrow red band. *Pink phycocyan* gives a similar kind of spectrum, only the principal absorption-band lies between the yellow and the green. It, too, is highly fluorescent, but the light of fluorescence is orange, due to a much broader band. These two mixed together constitute Cohn's phycocyan. Both are soluble in water, but exist in the plants in a solid form or concentrated solution, as shown by the position of the absorption-bands, which lie considerably nearer to the red end than when the corresponding substances are dissolved in water.

Phycoerythrine Group.

This is the name I propose to adopt for two colouring-matters found in *Algæ*, described by me in a former paper†. One is pink, giving a spectrum with an absorption-band at the yellow end of the green, and the other is red, the spectrum showing a band at the blue end of the green. I therefore call one *pink phycoerythrine* and the other *red phycoerythrine*. Neither are fluorescent, and both are soluble in water.

Erythrophyll Group.

This is the name I have applied to a large number of colouring-matters, soluble in water and insoluble in bisulphide of carbon, found in the leaves and petals of plants. I shall not now attempt to describe them in detail, but refer to my former papers for a further but yet very imperfect account of their peculiarities‡. They do not appear to be essential constituents of leaves, but rather products formed under particular conditions. Much remains to be learned respecting the different kinds; for though an accidental constituent, there are important differences between those formed under similar circumstances in the leaves or fronds of different

* Monthly Microscopical Journal, 1870, vol. iii. p. 229, and 1871, vol. vi. p. 124.

† Monthly Microscopical Journal, 1871, vol. vi. p. 124.

‡ Proc. Roy. Soc. 1867, vol. xv. p. 433. Quart. Journ. of Science, 1871, vol. i. p. 64. Quart. Journ. of Microscop. Science, 1871, vol. xi. p. 215.

classes of plants. On the whole they are very slowly changed by the action of light.

Chrysotannin Group.

I have also described this in the above-named papers. The various substances included in the group are all soluble in water, and of a more or less pale yellow colour; they are very slowly changed by the action of light. The connexion between them and the different classes of plants is still very obscure. They are interesting chiefly in connexion with the production of the tints of autumnal foliage, some of which are due to their oxidization.

Comparative Chromatology.

Having thus described the principal groups of colouring-matters, and those particular species which appear to be of the greatest importance in connexion with the present subject, I now proceed to the consideration of their distribution. The individual species are the elementary coloured constituents which exist in some and are absent from other plants, or occur in a very different proportion, according to the kind of plant or the conditions in which it grows. In my opinion it is only by a careful qualitative and quantitative determination of the coloured constituents that we can arrive at any satisfactory conclusions respecting them. Mere general colour is altogether deceptive: it might lead us to conclude that there was a complete difference in the colouring-matter, when there is merely a difference in the relative amount of the same substances; whilst, at the same time, it might appear to indicate that two cases were alike, when they really differ in most essential particulars. On the contrary, by such a method of qualitative and quantitative analysis the exact differences and the exact character of the connexion may be seen at a glance; and I cannot but think that by carrying out such principles our knowledge of certain questions connected with plants will be very greatly extended. The whole subject, however, is in its infancy; very much remains to be learned respecting nearly every thing connected with it; and what I now describe must be looked upon as very little more than a few general conclusions, which appear to follow from what is already known. One may fully expect that further research will much modify the explanations which now seem the most plausible; but it appears to me undesirable not to point out the general bearings of what is known, merely because they may be modified, or even disproved, by what is still unknown, since a mass of unconnected facts would be of very little interest.

The subject of comparative vegetable chromatology may be divided into two parts, viz.—first, that in which different specimens of the same plant growing in different conditions are compared, in order to learn the effects due to external agencies; and, secondly, that in which different plants growing in similar conditions are compared, in order to ascertain the effects due to internal organization.

Comparison of the same Plants growing in different conditions.

There are many facts connected with the colouring of plants which appear to be almost or quite unintelligible, if we suppose that the colouring-matters when once formed undergo no immediate further change: for example, if we suppose that the chlorophyll is formed in the young leaves in spring, and remains permanent until they fade in autumn. If, on the contrary, we assume as an hypothesis that the various colouring-matters (or at least some of them) are in a state of constant change, we can readily explain many striking facts. Whatever may be the ultimate conclusion, I shall therefore discuss my subject in accordance with the hypothesis that the colouring-matters are being more or less constantly formed by the internal constructive energy of the plants, and decomposed by the destructive action of the oxygen of the air, intensified by the presence of light; and that the condition of each particular part of a plant, at all seasons of the year and under different circumstances, depends upon and expresses the temporary or more permanent equilibrium between these two opposing forces, both of which may vary very considerably. By means of such an hypothesis it is possible to employ accurate physical methods in the study of vegetable physiology; and since the laws which regulate the destructive force may be determined by experimenting with dead matter, we may learn what does and what does not depend on life, and thus be in a better position to understand its essential nature.

According, then, to these principles we may expect to find in plants variations depending on a change in the constructive force, as well as others due to variations in the amount of light to which they are exposed.

Action of Light on Chlorophyll.

When a solution of chlorophyll is exposed to the sun and air it is decomposed, the rate of change and the character of the result varying much according to the solvent, and also depending on the presence of minute quantities of sensitizing or protecting oils. When dissolved in benzole or absolute alcohol, and protected by oil of citronelle, the chlorophyll slowly disappears, and very little of a red colouring-matter is formed, or, if it be formed, it is destroyed nearly as quickly, not accumulating so as to become conspicuous. On the contrary, when the chlorophyll is dissolved in bisulphide of carbon, the change is far more rapid, and several different crimson and red substances are formed, the production of each particular kind depending on a variety of conditions not yet completely understood; and this makes it still uncertain whether some are exclusively or only mainly due to the blue, and some to the yellow chlorophyll. Since these red substances are destroyed by the action of the blue rays, they are obtained to greater advantage by exposing to red light, which acts upon the chlorophyll, but not on these pro-

ducts. It thus appears that chlorophyll is in the first instance decomposed by light into various coloured compounds, which become colourless on further exposure, and that these may be developed so as to be very conspicuous or scarcely visible, according to particular conditions.

Production of Red Leaves.

The red substances thus formed artificially differ from those met with in plants in being soluble in bisulphide of carbon, and more rapidly destroyed by the action of light; but yet it appears to me extremely probable that those often found in leaves are products formed by the action of light on chlorophyll, under conditions which have not been reproduced artificially. At all events, by adopting this hypothesis, the changes that occur in many leaves at different seasons of the year admit of very simple explanation. I have constantly noticed that the leaves of some plants are more especially subject to this disappearance of chlorophyll and formation of erythrophyll when the twig on which they grow is partially broken, or when the leaf is actually severed from the plant, but remains in a damp place, so as not to actually wither, especially when the underside is exposed to the light. The change is also often dependent on the presence of parasitic fungi or insects, and often takes place along the dying edge of a leaf, where it is not actually dead, but, so to speak, only half alive; and hence there seems to be no doubt that one essential condition is that the leaf should be in a state of low vitality. It also depends upon the presence of light; for if a leaf that seems likely to change be partially covered with some opaque substance, that part will remain green when the rest has turned red. Such rough natural photographs are often found where one leaf has partially shaded another. This explanation of the facts agrees very well with what occurs at different seasons of the year. The leaves of some plants are almost perfectly green in summer, but as winter approaches they turn more or less red, and on the return of spring these self-same leaves lose their red colour and become green. From this I should draw the very simple and probable conclusion, that as the temperature falls the equilibrium between the constructive and destructive agencies is so much modified by the reduction of vital activity, that the amount of chlorophyll formed is relatively less than that of the red substance, whereas at a more favourable season of the year more of the chlorophyll and less of the red are produced, and the leaves again become green. On this principle it is also easy to understand the reason of the distribution of red colour in different parts of the same plant—as, for instance, only in the tips of the leaves or in the leaf-stalks, and in those parts well exposed to the sun. According to these views of the subject, permanent red varieties of plants are due to a similar difference in the equilibrium, sometimes greatly modified by the conditions in which they grow, the development of the erythrophyll being also generally dependent on exposure to light.

The fact of some kinds of leaves never turning red is analogous to what occurs in the case of particular solutions of chlorophyll. It is not at all improbable that in some cases certain kinds of erythrophyll may be formed independent of chlorophyll.

Waste and supply of the Colouring-matters in Plants.

As is well known, when plants grow in the dark very little or no chlorophyll and other colouring-matters are formed; they are, as it is said, *blanched*. Hence it appears that the production of such substances is some direct function of the action of light. At the same time chlorophyll when in solution is very rapidly decomposed by light, and the other colouring-matters are similarly affected, but in less and varying degree. The power with which they resist decomposition when in living plants is very remarkable, and may in part be due to their not being in solution, and to the unknown force which I have called constructive energy; but it appears to me very difficult or impossible to explain many facts, unless we suppose that light does to some extent exert this decomposing action on the colouring-matters, even when they exist in the leaves. Perhaps, indeed, it may be one of the most important changes that take place in them, and quite essential to plant life. It may seem strange to suppose that any thing can be formed by the action of light and also decomposed by it, but this corresponds with what does certainly occur in artificial experiments. The red substances formed from chlorophyll by the action of light are themselves afterwards decomposed by it, as I have already described; and if we suppose that chlorophyll and other colouring-matters are in some way or other formed through the agency of light from other constituents of the plants, and are afterwards decomposed by further action, we can easily explain a number of remarkable facts. It appears to me that their development is some direct function of light, until a certain quantity of each has been formed and a sort of equilibrium established, varying in its character according to the particular plant, but that at the same time the amount of each decomposed continues to increase directly as the intensity of the light; so that the equilibrium between the different substances is not the same for light of different intensities, but, after having reached a maximum, the quantity of each more or less decreases with increased light. This view of the subject seems justified by such an experiment as the following:—I selected two perfectly similar and contiguous leaves, nearly a year old, growing in the month of May on the outside of an *Acuba japonica*, where much exposed to the sun, and covered part of one with black cloth. After only a week the part of the leaf thus covered up and protected from the light had become most decidedly greener, and after three weeks it was a moderately deep green; whereas the part of the same leaf exposed to the light was still, as at first, the same very yellow-green as the whole of the other leaf which had not been covered up, the line of junction

being sharply defined. I then took exactly equal quantities of the part thus covered up and of the corresponding leaf which had remained the whole time exposed to the light, and determined the relative quantities of the chlorophylls, of the xanthophylls, and of a yellow colouring-matter soluble in water, by the quantitative methods already explained. Taking the amount of each at 100 for the leaf left in its natural exposed state, the relative quantities in that kept shaded were as follows:—

	Chlorophylls.	Xanthophylls.	Soluble in aq.
Natural state	100.	100	100
Shaded for three weeks	150.	75.	200

Here, then, we see that the chlorophyll had increased 50 per cent., which is an increase of above 2 per cent. per diem; and this, be it remembered, in a leaf nearly a year old. The increase in the yellow colour soluble in water was still greater, whereas the xanthophyll had diminished. This alteration may be greater than common, because I selected the plant on account of its being unusually sensitive to the action of light. Shading a leaf in the manner described above is of course quite a different thing to so smothering up an entire plant as to cause it to become sickly and fade, or to preventing the development of young leaves by keeping them in the dark. It seems to me that such a striking difference, about which there could be no doubt, must prove either that the absence of light is favourable to the development of chlorophyll, which would scarcely agree with other well-known facts, or else that the increase in its amount does in some measure indicate the quantity formed also in the other, but decomposed in the same interval by exposure to the sun. At all events the facts seem to prove that the equilibrium between the different constituents of leaves can soon be changed by altered conditions, as though, like the bodies of animals, the apparent permanence were only an approximately uniform equilibrium between construction and decomposition, the actual chemical changes being of course entirely different in the two cases. We may very well believe that the old and effete colouring-matters are thus destroyed, and that the result of this constant change is that the endochrome in the leaves is always in a young and vigorous condition. I use this term *endochrome* to signify any kind of mixed colouring-matter found in the cells.

Various comparative quantitative analyses.

This change of equilibrium is fully established by a comparison of leaves grown under different circumstances. The exterior leaves of *Acuba japonica*, on the south side, where fully exposed to the sun, are usually very much dappled with yellow patches almost free from chlorophyll, and even the green parts themselves are pale; whereas the leaves in the interior of the plant, where much shaded, have very few or none of the yellow patches, and are of a dark green. There is a similar differ-

ence in the general colour of the young leaves. I selected for comparative analysis leaves nearly a year old, representing a fair average of those on the outside and in the interior, and, by methods similar to those already described, I separated the colouring-matters into the mixed blue and yellow chlorophyll, orange xanthophyll, and the mixed xanthophyll, yellow xanthophyll, and lichnoxanthine, and obtained the following results:—

	Chlorophylls.	Orange xanthophyll.	Mixed xanthophylls &c.
In the shade	100	100	100
In the sun	31	90	100

I must here explain that in this and similar analyses the numbers in each column have no connexion with those in the other columns. For example, the 100 parts of chlorophyll have no relation to the 100 parts of xanthophyll; they do not indicate equal weights or equality in any other respect. If it were desirable to compare them together, it should, I think, not be by weight, but according to the total quantity of light which they absorb. I take 100 to express the maximum for each colouring-matter, and calculate out the relative amount of the same substance in the other specimen accordingly—that is to say, in this case the amount of chlorophyll in the leaves exposed to the sun is only 31 per cent. of that in those grown in the shade. The orange xanthophyll is in a similar manner reduced to 90 per cent., whereas the mixed xanthophylls &c. are as nearly the same in both as could be ascertained. This great reduction in the quantity of chlorophyll in the leaves exposed to the sun corresponds of course to the increase found in the case of the one covered up and protected from the light, but due to the permanent and normal difference in the amount of exposure to light of different parts of the plant. The effect of this in the case of *Acrida* is unusually great, and apparent at once on comparing the leaves; but I found that there was an analogous difference in the leaves of a deep green common holly (*Ilex aquifolium*), though it would scarcely have been noticed without special examination. I found the relative quantities to be as follows:—

	Chlorophylls.	Orange xanthophyll.	Mixed xanthophylls &c.
In the shade	100	100	100
In the sun	62	86	94

In the case of a lichen (*Platysma glaucum*) I found:—

	Chlorophylls.	Xanthophylls.	Lichno- xanthines.
In the shade	100	100	100
In the sun	75	77	100

In this case I did not compare equal weights of the fronds, but calculated out the results of the analyses on the supposition that the amount of the lichnoxanthines was the same in both.

For equal weights of the fronds of *Fucus serratus*, in spring, I found :—

	Blue chlorophyll.	Chlorofucine.	Orange xanthophyll.	Fucoxanthine.
In the shade ..	100	100	100	98
In the sun	79	87	90	100

Change in Equilibrium due to the Action of Light.

The principal conclusions to be drawn from the above facts are, that leaves exposed to the sun contain nearly (or, at all events, more nearly) the same quantity of the colouring-matters least changed by the action of light as those grown in the shade, but that when grown in the sun those substances which are more and more readily decomposed by the action of light are more and more reduced in amount in the same order. This difference in the equilibrium could be imitated artificially by so contriving that a certain quantity of each colouring-matter should be added at a uniform rate to a solution exposed to the sun. In that case the relative proportion in which they would remain would depend on the rate at which they were introduced and the intensity of the action of the light. Thus, making use of the same sort of rough illustration as that already adopted, we might have :—

Added in a given time	$10a + 10b + 10c$
Decomposed in the same time by a moderate light	$3a + 2b + c$
Equilibrium for a moderate light	$7a + 8b + 9c$
Decomposed by equal extra light	$3a + 2b + c$
Equilibrium for much light	$4a + 6b + 8c$

In the above illustration the amount supposed to be added at a uniform rate of course represents the quantity of each colouring-matter formed in the leaves in the time required to produce the corresponding decomposition. The results correspond so well that it seems to me a good provisional hypothesis; and the facts are of much interest, as showing that very important changes occur in living plants in accordance with the same laws as regulate the changes in the same colouring-matters when treated as simple dead compounds in experiment tubes.

Changes in Peltigera canina.

I now proceed to a more complicated case. The fronds of *Peltigera canina* contain at least seven different colouring-matters; but in my analyses I did not attempt to distinguish the two kinds of lichnoxanthine, and therefore give them as united. The apothecia are coloured by these latter substances, with little or no trace of any others. The results of

my analyses are calculated out, so as to compare the different specimens, on the supposition that the amount of orange xanthophyll is constant. I examined five examples of the plant, growing in very different situations, viz. :—

I. Where very damp and shady, so that no fructification was developed.

II. Where open to the sun, but slightly shaded by the grass amongst which it grew, and having fructification developed to great perfection, which of course was separated.

III. Rather more exposed to the sun, with no fructification.

IV. Still more exposed to the sun.

V. Very much exposed to the sun, growing on a bare rock, and being of much browner colour than I., II., and III., which were of a most decided green.

	Blue chlorophyll.	Chlorofucine.	Phyco- xanthine.	Orange xanthophyll.	Lichno- xanthines.
I.	61	100	65	100	40
II.	100	99	100	100	95
III.	86	73	79	100	100
IV.	43	54	76	100	71
V.	32	51	64	100	67

On comparing these together it will be seen that the specimen with such abundant fructification (II.) contained the maximum, or nearly the maximum, quantity of all the different colouring-matters, as though the amount of light were just that best suited for its growth. The specimen in extreme shade (I.) shows a remarkable falling off in some of the constituents, as if the plant were in a sickly state; and the small amount of the lichnoxanthines is particularly interesting, because they are the special colouring-matters of the fructification, which in this specimen was not developed, perhaps for that very reason. The difference in the proportion of the other substances in the specimens more and more exposed to the sun (III., IV. & V.) is in strict accordance with the laws already explained, and the reduction in the amount of chlorophyll is most remarkable. The lichnoxanthines, however, do not follow that law, but seem more connected with the presence or absence of fructification. In reference to this question I may mention that, as before named, the antherozoids of *Fucus serratus* are exclusively, or almost exclusively, coloured by orange xanthophyll, which occurs only in small quantity in the fronds themselves. Taking two portions of the plant, one from the lower part of a frond bearing many well-developed antheridia, and the other from a similar part of a frond of the same specimen with no antheridia, I found that the amount of orange xanthophyll was about 40 per cent. less in the former, as though this substance had been to a considerable extent drawn off from the frond to supply the antherozoids.

There is a decided indication of the same fact in the case of the lichen-xanthine in *Peltigera*, but scarcely sufficient to prove it conclusively.

The above examples must be looked upon merely as indications of what may be done; but they will, I trust, be sufficient to show that the study of the relative amount of the various colouring-matters found in the same plant, under different conditions, is likely to throw much light on certain questions connected with vegetable physiology.

Comparison of different Plants.

Though I have examined a good many of the principal classes of plants, yet there are many that I have not yet been able to procure in a proper state for examination. It is also desirable to investigate more fully certain of the lower classes of animals, especially the sponges and polyps, and to ascertain the laws regulating the distribution of some of the colourless constituents of plants, though that would require entirely different methods of study. Residing, as I do, inland has increased my difficulties, and I have been obliged to rely on what I could learn during a temporary residence in Devonshire, and from material subsequently sent to me by Dr. Baker of Dawlish, Mr. Charles Horner of Mortlake, and other kind friends. At the same time, being able to spend several hours every day in the open country, at all seasons of the year, has been of great value in studying other parts of my subject.

In the present state of the question there is some difficulty in deciding what general principles should be adopted in attempting to arrange the various classes in a natural order, in accordance with the nature or relative amount of the colouring-matters. For example, whether they should be arranged in a single linear series, or in two or more parallel or divergent groups. This difficulty is partly due to the existence of sudden gaps, not yet filled up by intermediate examples, so that the true connexion is not quite apparent. I have constructed a table, representing what is so far known; but I think it better not to publish it until more complete—only to describe some of the principal conclusions deduced from it.

Connexion of the different Groups of Algae.

Perhaps I cannot choose a better illustration than that furnished by the different groups of *Algae*—the olive, the red, and the green. They contain at least twelve different colouring-matters, distributed very differently, in such a manner as to connect, and yet to distinguish, the different groups very characteristically. I have not yet made any accurate quantitative analyses, and therefore express the relative amount of the various substances by the following signs:—

A relatively large quantity	*
A relatively moderate quantity	+
A relatively small quantity

	Olive.	Red.	Green.
Blue chlorophyll	+	+	*
Yellow chlorophyll			+
Chlorofucine	+	.	
Orange xanthophyll	+	+	+
Xanthophyll		+	+
Yellow xanthophyll			+
Fucoxanthine	*	.	
Lichnoxanthines	+
Phycocyan		+	
Pink phycocyan		*	
Red phycoerythrine		*	

On inspecting this Table it will be seen that the olive *Algæ* are characterized by the relatively large amount of chlorofucine and fucoxanthine, and the total absence of yellow chlorophyll, of xanthophyll, and of yellow xanthophyll. The red are especially distinguished by the colouring-matters of the phycocyan and phycoerythrine groups, but also differ from the olive in containing xanthophyll and very little chlorofucine and fucoxanthine. The green are characterized by the presence of yellow chlorophyll and yellow xanthophyll, as well as by the absence of chlorofucine, fucoxanthine, and the substances soluble in water, so characteristic of the red group. Blue chlorophyll, orange xanthophyll, and the lichnoxanthines are common to all. It will also be seen that the red group is intermediate between the olive and the green, and, independent of the red colouring-matters, it differs from each of the other groups far less than they do from one another. It is also still more closely connected with each by other examples. My endeavour has been to extend such a method of comparison to all the leading classes of plants and to some of the lower classes of animals, and to ascertain the order in which they should be arranged, so as, in like manner, to show the most gradual and unbroken passage from one to the other.

Connexion between the lowest classes of Animals and Plants.

Comparing these various groups of *Algæ* with other classes of plants, and with such low classes of animals as *Actiniae*, I found that the whole of the colouring-matters present in green *Algæ* are those most characteristic of all the higher plants, the only difference being that in certain circumstances these latter contain in addition various more or less accidental and unessential substances, belonging to the erythrophyll and chrysotannin groups, some kinds of which, nevertheless, do to some extent appear characteristic of particular classes. As far as their constituent colouring-matters are concerned, the green *Algæ* are therefore perfectly typical plants. On the contrary the olive *Algæ* differ in a very marked manner: they contain no yellow chlorophyll, nor either of the two kinds of

xanthophyll, all so characteristic of the most perfect plants, but contain chlorofucine and fucoxanthine, both of which occur in certain species of *Actiniae*, like *Anthea cereus*, var. *smaragdina*. The presence of such colouring-matters, therefore, connects the olive *Algæ* with the lower classes of animals, in the same manner that the presence of blue chlorophyll connects some animals with plants. Such substances, though essential to the growth of plants, are not constant in closely allied species of animals, as though they were of no more importance for the life of animals than the accidental vegetable colouring-matters are for the life of plants. The value of these connexions between plants and animals remains to be determined, but in any case such definite facts must, I think, have some very important signification. If, then, according to these principles, the olive *Algæ* be looked upon as a link connecting the lowest classes of plants with some of the lowest classes of animals, there is a perfect and simple continuity; whereas if they were to be considered intermediate between green *Algæ* and the higher *Cryptogamia*, there would be two great breaks of chromatological continuity.

Changes occurring in Oscillatoria.

The olive *Algæ* are also connected in another manner with lichens, through *Oscillatoria*. These latter plants are extremely interesting, since they are subject to most remarkable changes, depending on the conditions in which they grow. I have made a series of quantitative analyses, which show this in a striking manner. I may here say that the chief difficulty in the analyses was the determination of the amount of the lichnoxanthines in presence of chlorofucine and fucoxanthine, and therefore the quantities given must be looked upon as only approximate, derived from several different methods, none of which were perfectly satisfactory, though they all agreed in leading to the same general conclusions. In discussing the results of the analyses, it was requisite to take the amount of blue chlorophyll as uniform, since it was the only constituent occurring in any considerable quantity throughout the whole series. To have taken equal weights of the plants themselves would have been almost impossible, and would often have made those which really correspond very closely appear to differ extremely, since the constitution of the endochrome is the important question. Of course by thus calculating the results as if the amount of chlorophyll were the same in all, there appears to be an increase in some of the other constituents in the specimens exposed to the sun, due, however, in reality to a reduction in the relative quantity of chlorophyll.

For comparison I give the following:—

I. *Fucus serratus* grown in the shade.

II. The same plant grown in the sun.

III. *Oscillatoria* grown under water, in a cold spring, in a very shady place.

IV. The same plant, in the same spring, where more exposed to light.

V. The same plant, growing in and on the surface of water, where fully exposed to direct sun.

VI. Probably a different species of *Oscillatoria*, growing on a damp wall, completely exposed to the sun.

VII. *Peltigera canina*, slightly shaded and having much fructification.

VIII. The same plant, where much exposed to the sun.

		Blue chloro- phyll.	Chloro- fucine.	Phyco- xanthine.	Orange xantho- phyll.	Fuco- xanthine.	Lichno- xanthines.
<i>Fucus.</i>	I.	100	90	0	3	77	11
	II.	100	100	0	3½	100	14
<i>Oscillatoria.</i>	III.	100	13	0	1	51	6
	IV.	100	19	36	3	55	10
	V.	100	trace	67	25	11	9
	VI.	100	„	100	77	25	23
<i>Peltigera.</i>	VII.	100	„	27	32	0	32
	VIII.	100	„	54	100	0	100

In this Table are compared together the same or very similar plants growing in different conditions, as connected by the brackets, and also plants belonging even to different classes. On comparing together the amount of the different constituents of the same plants grown in less or more light, it will be seen that some of the differences are in perfect agreement with those already described; but the differences in the *Oscillatoria* are evidently not a mere change in equilibrium, due to the decomposing action of the light, and point unmistakably to a great difference in the constructive force of the plant, depending on increased light. There is a remarkable development of phycocyanine and orange xanthophyll, and a great decrease in the amount of chlorofucine and fucoxanthine, and the result is that we have a change almost from the type of olive *Algae* to that of certain lichens. When growing in a very shady place the colouring-matters soluble in bisulphide of carbon are all identical with those in *Fucus* and other olive *Algae*, whereas when grown exposed to much sun there is a great reduction in the amount of those substances which are so characteristic of that group, and at the same time a great development of others which are almost or altogether absent from it, but occur in large quantity in, and are very characteristic of, such lichens as *Peltigera canina*. The olive *Algae* are, however, distinguished from those *Oscillatoria* which approach them most closely by the absence of the phycocyanins; and though these occur in *Peltigera*, it

The colouring-matters found in *Actinæ* are very various, and it is only particular species that contain those found in *Algæ*. Lichens, as a whole, are characterized by a number of what may be called accidental constituents—such, at least, as occur in one species and not in another closely allied to it. Many of these are almost or quite colourless substances, which easily give rise to colouring-matters when treated with various reagents. This fact, combined with their partial distribution, is taken advantage of in studying lichens as a means for distinguishing closely connected species. Their more constant and apparently fundamental colouring-matters correspond with those found in the higher classes of plants, but differ considerably in relative proportion, the lichnoxanthines usually being relatively more abundant.

Relation of Fungi to other Plants.

Fungi cannot be arranged in a direct order in any part of the series shown in the Table given above. Their most common colouring-matters exactly correspond with those found in the apothecia of lichens, and their more accidental constituents are also quite analogous to those occasionally found in the apothecia of particular lichens—for example in those of *Cladonia cornucopioides*.

According to the principles adopted in this paper, fungi ought then to be looked upon, not as fronds, but as the fructification of a low type of plants; and I think that the fact of the colouring-matters alone leading to such a satisfactory conclusion, shows that they must have some important physiological signification.

Connexion between the Colouring-matters of Flowers and those in the Leaves.

Since a comparison between the colouring-matters found in the petals of flowers with those in the foliage has many points of interest in connexion with the relationships of fungi, it seems best now to describe some of the principal facts. The coloured substances in the petals are in many cases exactly the same as those in the foliage from which chlorophyll has disappeared; so that the petals are often exactly like leaves which have turned yellow or red in autumn, or the very yellow or red leaves of early spring, but in other cases special coloured substances are developed which do not occur in leaves. Many yellow flowers are coloured by a very variable amount and variable mixture of lichnoxanthine and the three kinds of xanthophyll, but orange-coloured flowers sometimes contain in addition other substances analogous to peizixanthine and phycoxanthine. The colour of many crimson, pink, and red flowers is due to the development of substances belonging to the erythrophyll group, and not unfrequently to exactly the same kind as that so often found in leaves. The number of different species is very considerable, and it would make this paper far too long to describe them; and, besides this, it would be requisite to explain methods of study

differing entirely from those given above. Some of the particular colouring-matters occur in very different natural orders; whereas I have found others only in one particular order, or even only in a single species of plant. Much remains to be learned in connexion with this branch of the subject; but many facts seem to indicate that these various substances may be due to an alteration of the normal constituents of leaves, some being probably formed from chlorophyll, others from the xanthophylls, and perhaps some from other constituents. So far as I have been able to ascertain, their development seems as if related to extra oxidization, modified by light and other varying conditions not yet understood; and it will be an interesting field for inquiry to determine how far they are due to simple changes in the normal constituents of leaves—in fact how far the petals of flowers are modified leaves, in colour as well as in structure.

Effects of Light on Flowers.

I have made very few experiments with the view of ascertaining the effect of a different amount of light on the petals of flowers, but what I have thus learned in the case of a dark variety of the common wallflower (*Cheiranthus cheiri*) clearly indicates that very important facts could be learned in this manner. By diminishing the exposure to light, a complete alteration was produced in the relative amount of the colouring-matters. No erythrophyll was developed, which was abundant in those flowers exposed to the sun, and, for an equal quantity of petals, only about one third the amount of xanthophyll, and yet about the same quantity of chlorophyll and lichnoxanthine were formed. The result was that I had produced a temporary alteration in the proportion of the colouring-matters, which corresponded closely with what is the permanent condition of a different natural variety or even of a different but closely allied species. Judging from certain facts which have lately attracted my attention, I think it extremely probable that in some cases natural varieties may be imitated in a similar temporary manner by reducing the constructive energy of the plants by other means. The extension of such inquiries will probably throw much light on the cause of the production of different coloured varieties of the flowers of the same species of plant, and show that some, at all events, may be due to permanent arrested or increased development of a particular kind.

Connexion between Fungi and Lichens.

Such, then, being the relation between the organs of reproduction and the foliage, it is to some extent possible to understand the connexion between parasitic plants like fungi, which do not derive their support from the constructive energy of their own fronds, and those which are self-supporting and possess true fronds. In the highest classes of plants the flowers are connected with the leaves more especially by means of xanthophyll and yellow xanthophyll; whereas in the case of lichens the

apothecia contain very little, if any, of those substances, but a large amount of the lichnoxanthines so characteristic of the class. Looking upon fungi from this chromatological point of view, they bear something like the same relation to lichens that the petals of a leafless parasitic plant would bear to the foliage of one of normal character—that is to say, they are, as it were, the coloured organs of reproduction of parasitic plants of a type closely approaching that of lichens, which of course is in very close, if not in absolute agreement with the conclusions drawn by botanists from entirely different data.

Yellow varieties of Leaves.

One of the most interesting questions connected with the higher classes of plants is the production of yellow varieties of leaves. Some, instead of being of the usual deep green, are far more yellow, or have special leaves or parts of leaves containing a very little chlorophyll generally diffused. Such leaves must be distinguished from those of normal green character with very bright patches free from chlorophyll, like *Croton variegatum*. Now I find that in many, if not all, such cases there is not only a great change in the total amount, but also a most important difference in the relative quantity of the blue and the yellow chlorophyll. This may be seen by examining the spectra of the leaves themselves, as illuminated by such bright, direct, concentrated sunlight as will penetrate through so many thicknesses of the leaves, pressed together by a strong compressorium, and show the spectrum to advantage. If in this manner, for example, ten thicknesses of very yellow leaves be compared with a single green one, it may be seen that the absorption-bands in the red due to the chlorophylls differ most materially. By proper management the band in the red due to yellow chlorophyll can always be seen in the spectrum of normal green leaves, but no trace is visible in the case of these very yellow leaves—only that due to blue chlorophyll. I have confirmed this conclusion by examining the chlorophyll when in solution in benzole, and find that, though some of the yellow kind is present, the relative amount is much less than normal. The method of study which I have adopted is as follows:—I crush the leaves, and heat them in spirit until all the chlorophyll is dissolved. When cold I agitate with bisulphide of carbon, adding so much water that the whole of the chlorophylls may be carried down; and after evaporating the bisulphide at a gentle heat, I redissolve in benzole. Taking two tubes containing a solution of such a strength that the spectrum shows the absorption-band in the red of the yellow chlorophyll to the greatest advantage, I add benzole to one tube until the band in the red due to blue chlorophyll is of the same intensity as that due to the yellow chlorophyll in the other tube, as seen by comparing the spectra together side by side with equally bright illumination, and carefully measure how much the original

solution had to be diluted in order to obtain that equality. This being a constant point of comparison, it is possible to determine the relative quantity of yellow chlorophyll in different kinds of leaves. So far I have not worked out the question in full detail, because the present form of instrument and apparatus is not suited for very accurate determinations, and therefore the following Table must be looked upon as a mere first attempt. Great care must be taken not to expose such dilute solutions to much light, since they are so rapidly changed, even by gaslight, that serious errors might easily result from this cause. In all cases I have taken the amount of blue chlorophyll as 100, and expressed the relative quantity of yellow chlorophyll in percentages, in accordance with the above named principle. I also give some other cases for comparison.

	Blue chlorophyll.	Yellow chlorophyll.
Olive and red <i>Alge</i>	100	0
Leaves grown nearly in the dark	100	5 or 6
Normally yellow leaves grown in the light..	100	5 or 6
Healthy green leaves	100	13 to 17
Green leaves faded to yellow	100	25 or 30
Final equilibrium in solutions exposed to the sun	100	near 100

The normal relative amount of yellow chlorophyll in green leaves certainly varies, and there seems reason to believe that this to some extent, if not mainly, depends on the length of time to which they have been exposed to the sun. When healthy green leaves fade and turn yellow the relative amount of yellow chlorophyll is increased, in accordance with what is known respecting the greater ease with which blue chlorophyll is decomposed, but the relative amount does not appear to become so great as in the case of a mixed solution exposed to the sun. On the contrary, when leaves are very yellow, owing to having been grown almost in the dark, the relative amount of yellow chlorophyll is much less than normal, as though under such unfavourable conditions the blue chlorophyll were more readily formed than the yellow. It might therefore be said that when the constructive energy is weak, the initial proportion of the yellow is about one third of the normal, but that when the leaves fade, the vanishing ratio is about double the normal. The blue chlorophyll is therefore, as it were, more readily formed and more readily decomposed than the yellow. The relatively small amount of yellow chlorophyll found in those leaves which are normally very yellow cannot therefore be due to the decomposing effect of light, but must rather be attributed to weak constructive energy, like that in leaves abnormally yellow on account of having been grown almost in the dark. This great reduction in the relative quantity of yellow chlorophyll causes them to approach towards the normal type of those low orders of plants from which it is altogether absent. The determination of the relative amounts of the two

kinds of xanthophyll is often rather difficult; but in some cases, at all events, the relative quantity of the yellow xanthophyll is greatly reduced in yellow varieties of leaves, so that, as in other circumstances, it seems to vary in the same manner as yellow chlorophyll. This question, however, requires further examination. The very great relative amount of the xanthophylls is readily explained by supposing that they are formed when the constructive energy is too low to give rise to chlorophyll, and that they may remain in faded leaves when the energy is too weak to prevent its decomposition.

Condition of Chlorophyll in Leaves.

By carefully studying the position of the principal absorption-band in the red, seen in the spectra of the leaves themselves, by means of a compound prism of considerable dispersive power, and a bright dot, seen by reflection from the upper surface, made to move over the spectrum by a micrometer-screw, as proposed by Mr. Browning and modified by myself, I find that there is every reason to believe that in normal healthy green leaves the chlorophyll is chiefly in a free state. On boiling for a short time in water, the oils or wax present in the leaf combine with the chlorophyll, and raise the band somewhat further from the red end, especially if the relative amount of chlorophyll is small. Of course I here refer to leaves which have little or no free acid in the juice to decompose the chlorophyll into the well-known product of the action of acids. When dissolved in the common fixed oils the band is still further raised. The small quantity of chlorophyll in very yellow leaves appears to be chiefly combined naturally with some oil, fat, or wax; so that the spectrum resembles that of a green leaf that has been boiled. This may perhaps be due in part to the total amount being so small in comparison to that of the oil or wax, but at the same time it is interesting to find that a relatively large quantity is thus combined in olive *Algæ*; and it will be a subject for further inquiry to ascertain whether, when chlorophyll occurs in animals, it is not similarly combined, or even dissolved, in a fat or oil.

Taking, then, every thing into account, leaves which are normally very yellow appear to be characterized by a very low constructive energy—so low, in fact, that a plant could not live if all its leaves were of the extreme condition of that type; and this, like what was described in connexion with *Oscillatoria*, seems to point to the conclusion that those classes of plants (like the red and olive *Algæ*) which more or less closely resemble these yellow leaves in certain important particulars are lower in the scale as regards some peculiarity on which the production of the colouring-matters depends, which, for want of a better name, I have called constructive energy. We might thus speak of constructive energy as being weak or strong in the same plant in different conditions, or as being of a low or high type in different classes of plants; and such

cases as those just named seem to show that a weak constructive energy has a great tendency to give rise to results analogous to those due to one of low type. If this conclusion should be confirmed by further research, it would be an important fact in connexion with the theory of evolution, since it would show that, as in the organization of animals, where development is arrested, there is a more or less permanent continuance of a lower type of structure, so in plants there is a permanent continuance of a lower type of colouring.

Conclusion.

Such, then, is a general account of the present state of what I have named comparative vegetable chromatology. I cannot but feel that it is very incomplete, both in compass and in detail, and that much remains to be learned respecting nearly every thing connected with it. Not only are the natural objects requiring careful study very numerous and often difficult to obtain in a proper condition, but, as will have been seen, several new questions are most intimately connected with my subject—questions of great interest in connexion with optics and chemistry, which would probably never have been raised if it had not been for their relation to the facts I have described. There are indications of other general questions which have not yet been fairly examined, and, in fact, the whole subject must be looked upon as being quite in its infancy. At the same time I trust that what I have described will be sufficient to show that the nature and relative proportion of the various colouring-matters in plants must have some very important signification, and that a more complete knowledge of comparative vegetable chromatology may be expected to throw much light on the development of plants, and enable us to examine some of the most fundamental questions in biology from a new and independent point of view. The subject is also interesting in other respects. The storing up of the energy of the sun's rays in the various compounds formed by plants is probably so intimately connected with the optical and chemical properties of some of their coloured constituents, that the further extension of such inquiries as I have described may possibly assist in clearing up this very difficult and yet most important problem.

XXIII. "On the Action of Electricity on Gases.—No. II. On the Electric Decomposition of Carbonic-Acid Gas." By Sir B. C. BRODIE, Bart., D.C.L., F.R.S., late Waynflete Professor of Chemistry in the University of Oxford. Received June 19, 1873.

(Abstract.)

In my previous experiments the maximum amount of ozone obtained by the action of electricity upon pure oxygen passed through the induction-tube of W. Siemens was about 20 per cent., an amount which, under

the conditions of the experiment, could not be exceeded. It occurred to me as possible to replace the 80 per cent. of oxygen unaffected by the action of the electricity by an indifferent gas, and thus to effect the complete conversion of oxygen into ozone. This idea was the starting-point of the following investigation.

When pure and dry carbonic-acid gas is subjected in the induction-tube to the electric action, a certain proportion of the gas is decomposed into carbonic oxide and oxygen, a portion of which appears in the form of ozone. The presence of this ozone may be detected by its odour and the characteristic oxidations produced by it.

It was a point of fundamental importance to determine whether the ozone thus generated was the same in kind as that formed by the action of electricity on pure oxygen, and whether the oxidations referred to were exclusively due to it; for it was quite conceivable that under the peculiar circumstances of the experiment other substances capable of producing similar oxidizing effects might be formed. Now, in the investigation before referred to, I had discovered certain quantitative reactions of ozone by which this substance is discriminated from all other known gases, and by which its presence may be detected and its quantity estimated. The electrized carbonic-acid gas was examined by the aid of these reactions.

When the electrized gas was passed through a solution of hyposulphite of soda, it was found to undergo a diminution in volume equal in amount to twice the "iodine-titre" of the gas, the mean of twelve experiments giving for this value 1.9, the "titre" being 1. The oxidation also effected in the solution of hyposulphite of soda was equal to three times the "titre" of the gas, the mean of a set of five experiments giving for this value, according to one method of estimation, 2.72, and according to a second and more exact method 3.01. Again, the oxidation effected by the electrized gas in a solution of protochloride of tin was equal to three times the "titre" of the gas, the value found as the mean of six experiments being 2.77. In two experiments also, in which both the oxidation and contraction were respectively estimated, 2.75 and 2.69 were the values found for the oxidation, and 1.9 and 1.8 the values found for the contraction, the "titre" of the gas being in all cases assumed as 1. These experiments conclusively demonstrate the identity of the ozone formed by the action of electricity upon carbonic-acid gas with the ozone similarly generated from pure oxygen.

Various experiments were instituted with the view of ascertaining the conditions most favourable to the conversion of a large proportion of this oxygen into ozone. The conclusion at which I arrived was that the greatest proportion of ozone in relation to the total oxygen was produced when a rapid current of dry carbonic-acid gas was acted upon at a low temperature by electricity of feeble tension. When these conditions were realized, it was found that 75 per cent. of the total oxygen

eliminated in the induction-tube by the decomposition of the carbonic-acid gas could readily be converted into ozone, but that it was difficult to pass this limit. This amount corresponds with the formation of a gas constituted of ozone and oxygen in the proportion of two units of ozone to one unit of oxygen, the matter of the oxygen being thus distributed $2\xi^3 + \xi^3$. In several experiments, nevertheless, this limit was considerably exceeded; and a table is given of nine experiments, in which the amount per cent. of ozone ranged from a minimum of 76.6 per cent. to a maximum of 85.5 per cent. This latter amount corresponds with a gas constituted of ozone and oxygen in the proportion of four units of ozone to one of oxygen, thus $4\xi^3 + \xi^3$. In this case the ratio of the total oxygen present to the titre of the gas is 3.5; if the whole oxygen were converted into ozone, the value of this ratio would be 3.

These experiments, taken in connexion with those described in my previous communication, leave no room for reasonable doubt as to the composition of the unit of ozone, which is the triad form of the element oxygen, and is to be symbolized as ξ^3 , an hypothesis henceforth to be regarded as conclusively demonstrated. This remarkable discovery has many important bearings upon chemical theory, especially in relation to the problem elsewhere raised by me of the true nature of the elemental bodies and the constitution of the unit of the element chlorine, which ozone so closely resembles in its chemical properties, and which is also a triad element symbolized as $\alpha\chi^3$.

Presents received May 29, 1873.

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- Martins (Charles) *Comparison des membres pelviens et thoraciques chez l'Homme et chez les Mammifères, déduite de la torsion de l'humérus.* 8vo. *Paris* 1873. *Statistique des Savans.* 8vo. 1873. The Author.
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Photographic Portrait of Thomas Davidson, F.R.S., by Maull & Co.

T. Davidson, Esq.

Photograph of R. J. Newall's Refracting Telescope, 1872.

R. S. Newall, Esq.

November 20, 1873.

Sir GEORGE BIDDELL AIRY, K.C.B., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Capt. John Herschel, R.E., elected in 1871, was admitted into the Society.

General Boileau, Mr. Etheridge, Mr. Merrifield, Mr. Newmarch, and Prof. Ramsay, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the Table, and thanks ordered for them.

The following communications were read :—

I. "Note on the Electrical Phenomena which accompany irritation of the leaf of *Dionæa muscipula*." By J. BURDON SANDERSON, M.D., F.R.S., Professor of Practical Physiology in University College. Received October 13, 1873.

1. When the opposite ends of a living leaf of *Dionæa* are placed on non-polarizable electrodes in metallic connexion with each other, and a Thomson's reflecting galvanometer of high resistance is introduced into the circuit thus formed, a deflection is observed which indicates the existence of a current from the proximal to the distal end of the leaf. This current I call the *normal leaf-current*. If, instead of the leaf, the leaf-stalk is placed on the electrodes (the leaf remaining united to it) in such a way that the extreme end of the stalk rests on one electrode and a part of the stalk at a certain distance from the leaf on the other, a current is indicated which is opposed to that in the leaf. This I call the *stalk-current*. To demonstrate these two currents, it is not necessary to expose any cut surface to the electrodes.

2. In a leaf with the petiole attached, the strength of the current is determined by the length of the petiole cut off with the leaf, in such a way that the shorter the petiole the greater is the deflection. Thus in a leaf with a petiole an inch long, I observed a deflection of 40. I then cut off half, then half the remainder, and so on. After these successive amputations, the deflections were respectively 50, 65, 90, 120. If in this experiment, instead of completely severing the leaf at each time, it is merely all but divided with a sharp knife, the cut surfaces remaining in accurate apposition, the result is exactly the same as if the severance were complete; no further effect is obtained on separating the parts.

3. *Effect of constant current directed through the petiole on the leaf-current.*—If the leaf is placed on the galvanometer electrodes as before,

and the petiole introduced into the circuit of a small Daniell, a commutator being interposed, it is found that on directing the battery-current down the petiole (i. e. *from* the leaf), the normal deflection is increased; on directing the current *towards* the leaf, the deflection is diminished.

4. *Negative variation.*—*a.* If, the leaf being so placed on the electrodes that the normal leaf-current is indicated by a deflection *leftwards*, a fly is allowed to creep into it, it is observed that the moment the fly reaches the interior (so as to touch the sensitive hairs on the upper surface of the lamina), the needle swings to the right, the leaf at the same time closing on the fly.

b. The fly having been caught does not remain quiet in the leaf; each time it moves, the needle again swings to the right, always coming to rest in a position somewhat further to the left than before, and then slowly resuming its previous position.

c. The same series of phenomena present themselves if the sensitive hairs of a still expanded leaf are touched with a camel-hair pencil.

d. If the closed leaf is gently pinched with a pair of forceps with cork points, the effect is the same.

e. If the leaf-stalk is placed on the electrodes, as before, with the leaf attached to it, the deflection of the needle due to the stalk-current is *increased* whenever the leaf is irritated in any of the ways above described.

f. If half the lamina is cut off and the remainder placed on the electrodes, and that part of the concave surface at which the sensitive hairs are situated is touched with a camel-hair pencil, the needle swings to the right as before.

g. If, the open leaf having been placed on the galvanometer electrodes as in *a*, one of the concave surfaces is pierced with a pair of pointed platinum electrodes in connexion with the opposite ends of the secondary coil of a Du Bois-Reymond's induction apparatus, it is observed that each time that the secondary circuit is closed the needle swings to the right, at once resuming its former position in the same manner as after mechanical irritation. No difference in the effect is observable when the direction of the induced currents is reversed. The observation may be repeated any number of times; *but no effect is produced unless an interval of from ten to twenty seconds has elapsed since the preceding irritation.*

h. If the part of the concave surface of the leaf which is nearest the petiole is excited, whether electrically or mechanically, the swing to the right (negative variation) is always preceded by a momentary jerk of the needle to the left, i. e. in the direction of the deflection due to the normal leaf-current; if any other part of the concave surface is irritated, this does not take place.

i. Whether the leaf is excited mechanically or electrically, an interval of from a quarter to a third of a second intervenes between the act of irritation and the negative variation.

II. "On the Algebraical Analogues of Logical Relations." By
ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A., F.C.P.S., F.C.P.
Received March 19, 1873.

(Abstract.)

The object of this paper is to examine the "mathematical theory of logic," thus laid down by Dr. George Boole in his 'Laws of Thought,' p. 37:—"Let us conceive of an Algebra in which the symbols x, y, z , &c. admit indifferently of the values 0 and 1, and of these values alone. The laws, the axioms, and the processes of such an algebra will be identical in their whole extent with the laws, the axioms, and the processes of an Algebra of Logic. Difference of interpretation will alone divide them." For this purpose, first the laws of such an algebra have been investigated independently of logic; and secondly the laws of primary and secondary logical propositions as laid down by Dr. Boole have been developed in an algebraical form, and compared with the former. The main results presumed to be established are:—

1. That there is a fundamental difference between such an algebra and logic, inasmuch as the algebra admits of only *two* phases, 0 and 1, and logic admits of *three* phases, namely, not only *none* and *all*, corresponding to 0 and 1, but also *some*, "which, though it may include in its meaning *all*, does not include *none*" (*ibid.* p. 124), and hence has no analogue in such an algebra; that is, an algebra of 0 and 1 can correspond only to a logic of *none* and *all*.

2. That, notwithstanding this difference, there are certain formal relations of equations which allow the algebra of 0 and 1 to be used as an *algorithm* for the purpose of arriving at certain logical forms, which, however, have then to be interpreted on a basis which has not even any analogy to the algebraical.

3. That the introduction of this algorithm introduces theoretical difficulties, adds to the amount of work, and is entirely unnecessary even for the purposes of the theory of probabilities founded upon it by Dr. Boole.

In the paper the general case is examined, and a solution is proposed for the difficulties arising from the interpretation of the symbols $\&$ and $\&$, which Boole says "must be established experimentally" (*ibid.* p. 92). The following simple case will show the nature of the investigation; but, for brevity, even this case is not fully considered.

In *algebra*, let x and y have either one of the values 0 or 1, and no other. And let x', y' be connected with them by the equations

$$1 = x + x' \quad \text{and} \quad 1 = y + y'. \quad \dots \dots \dots (a)$$

Then $xx' = 0, \quad \text{and} \quad x = x^2; \quad \dots \dots \dots (b)$

and by simple multiplication

$$x = xy + xy', \quad x' = x'y + x'y', \quad \dots \dots \dots (c)$$

$$1 = xy + xy' + x'y + x'y'. \quad \dots \dots \dots (d)$$

In (d) one term on the right *must* = 1, and each one of the rest *must* = 0. This has no logical analogue. The ambiguity is diminished by taking some relation between x and y , as $x = xy$, which by (c) gives $xy' = 0$, and hence, by (d),

$$1 = xy + x'y + x'y', \quad \dots \dots \dots (e)$$

showing that one of three (instead of four) terms *must* = 1, and each of the other two *must* = 0. This has also no logical analogue. But in (e) we find that x occurs only in the term xy , and y' only in the term $x'y'$, whereas x' and y' both occur in two terms. This is the only relation useful in logic. But further, owing to the necessity for all terms vanishing except one, there can be only one term in which x occurs, in which case neither x' nor y' can occur, and there may be no such term. This again has no logical analogue.

In *logic* (only a primary proposition being considered for brevity) let U be the things themselves in the "universe of our discourse" (*ibid.* p. 44), X those among them having the name X_a and attribute X_a , and similarly for Y, Y_a, Y_a . Also let X', Y' be those things among them which are not X and Y . Let XY mean those things X which are also things Y , and YX those things Y which are also things X . Let $P = Q$ mean that the group of things P is the same as the group of things Q . Then, disregarding the order in which attributes occur, and the number of times that they recur, $XY = YX$, and $X = XX$. Let νP represent the number of the things P , then

$$\nu U = \nu X + \nu X', \nu U = \nu Y + \nu Y', \nu XX' = 0, \nu X = \nu XX, \quad \dots (a', b')$$

$$\nu X = \nu XY + \nu XY', \nu X' = \nu X'Y + \nu X'Y', \quad \dots \dots (c')$$

$$\nu U = \nu XY + \nu XY' + \nu X'Y + \nu X'Y', \quad \dots \dots \dots (d')$$

which are of the same form as (a, b, c, d). But in (d'), although one term on the right *may* = νU , in which case each of the rest *must* = 0, most generally *no* term on the right of (d') will = νU , and *any number* of the terms *may* be greater than 0. This has no analogue in the algebra in question. If there is a limiting equation as $X = XY$, or "all X is Y ," giving $\nu XY' = 0$ from (c'), or "there is no X which is not Y ," then (d') reduces to

$$\nu U = \nu XY + \nu X'Y + \nu X'Y', \quad \dots \dots \dots (e')$$

and all the X which exist (there may be none) are Y , and *all* the Y' which exist (there may be none) are X' . This has the algebraical analogue already mentioned. But there may be *both* X and Y' , in which case *neither* νXY nor $\nu X'Y'$ either = νU or = 0, and this has no algebraical analogue. Again, whether either or neither of νX and $\nu Y' = 0$, $\nu X'Y$ may be greater than 0; and this has no algebraical analogue—although, if *both* $\nu X = 0$ and $\nu Y' = 0$, then $\nu X'Y = \nu U$, which has an algebraical analogue.

The difference between the laws of such an algebra and of logical calculation, therefore, appears to be one of principle and not of interpretation only.

November 27, 1873.

W. SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows :—

President.—Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— { Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.
 { Prof. Thomas Henry Huxley, LL.D., Ph.D.

Foreign Secretary.—Prof. Alexander William Williamson, Ph.D.

Other Members of the Council.—Sir George Biddell Airy, K.C.B., M.A.; Sir B. C. Brodie, Bart, M.A., D.C.L.; Prof. Arthur Cayley, LL.D.; John Evans, Sec. G.S., F.S.A.; Daniel Hanbury, Treas. L.S.; Nevil Story Maskelyne, M.A.; Prof. James Clerk Maxwell, M.A.; C. Watkins Merrifield, Hon. Sec. I.N.A.; Joseph Prestwich, V.P.G.S.; Andrew Crombie Ramsay, LL.D.; Rear-Admiral G. H. Richards, C.B.; Prof. George Rolleston, M.D., M.A.; Prof. J. S. Burdon Sanderson, M.D.; William Sharpey, M.D., LL.D.; Francis Sibson, M.D.; Major-Gen. R. Strachey, R.E., C.S.I.

Dr. Aitken, Dr. Paget, and Lord Rayleigh were admitted into the Society.

Pursuant to notice given at the last Meeting, Baron Jean Baptiste Julien D'Omalius d'Halloy of Brussels, Georg Adolph Erman of Berlin, Asa Gray of Cambridge, Massachusetts, Franz Gustav Jakob Henle of Göttingen, Charles Hermite of Paris, and Otto Struve of Pulkowa, were balloted for and elected Foreign Members of the Society.

The Presents received were laid on the Table, and thanks ordered for them.

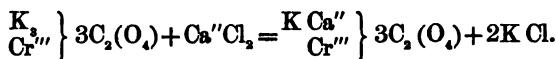
The following communications were read :—

- I. "On the Optical Properties of a new Chromic Oxalate." By WALTER NOEL HARTLEY, F.C.S., Demonstrator of Chemistry, King's College, London. Communicated by Dr. GLADSTONE, F.R.S. Received June 19, 1873.

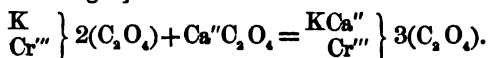
With the view of making a series of spectroscopic observations on coloured salts, I have lately prepared many salts of chromium, and have been successful in obtaining some interesting new substances, one of which is remarkable alike in its chemical constitution, its fine crystalline cha-

racter, and its optical properties. It has the formula $\left. \begin{smallmatrix} \text{KCa''} \\ \text{Cr'''} \end{smallmatrix} \right\} 3\text{C}_2\text{O}_4 \cdot 4\text{H}_2\text{O}$, and must therefore be called *potassio-calcium chromic oxalate*.

Preparation.—*a.* By mixing the blue potassium chromic oxalate with calcium chloride: the solutions should be dilute and warm. The salt is deposited in crystals.



b. By boiling a solution of red potassium chromic oxalate with freshly precipitated calcium oxalate, a solution is made which deposits the new salt, after the following equation:—



The former method is preferable.

It was noticed that the crystals became opaque and of a greenish-violet tint when dried over sulphuric acid, though their general appearance is of a brilliant greenish-black lustre. A basinful of crystals was then divided, one portion being dried by pressure between folds of filter-paper, and the other by standing in a desiccator for thirty hours; the difference in appearance previously noticed became evident. Desiccation *in vacuo* over sulphuric acid, as well as heating to 100° C., renders these crystals anhydrous; and they have then a decidedly violet colour, with a silky lustre; exposure to the air even while being weighed renders them green again, the green tint commencing at the edges, which shows that they readily reabsorb water. The analysis of the salt was made by fusing it with $2\frac{1}{2}$ times its weight of a mixture of equal parts of sodium carbonate and potassium nitrate. The filtrate was in some cases neutralized with acetic acid, acidulated with nitric acid, and the chromium precipitated as mercurous chromate, and ignited as usual; in other cases the chromate was reduced with a drop or two of an alcoholic solution of sulphurous oxide, and the sesquioxide precipitated by ammonia. The potassium was estimated by fusing the salt alone, dissolving in hydrochloric acid, and precipitating with platinic chloride.

Analysis.

	Weight of salt. grm.	Dried over H_2SO_4 .	H_2O lost.
I.	1	0.961	3.9 per cent.
II.	1	0.960	4.0 ,,

Heated to 100° C.

		H_2O lost.	Total water.
I.	0.879	12.1 per cent.	16.00 per cent.
II.	0.879	12.1 ,,	16.10 ,,

	I. grm.	II. grm.	III. grm.	IV. grm.
Weight of salt taken ..	1.953	1.78	1.34	1.00
Found.				
Ca CO ₃	0.4163	0.3797	0.2842	—
Cr ₂ O ₃	0.3062	0.2823	0.1942	—
K	—	—	—	0.08*

	Percentage composition.			
	I.	II.	III.	IV.
Ca	8.52	8.53	8.48	—
Cr ₂ O ₃	15.67	15.85	15.13	16.38
H ₂ O	16.0	16.1	16.0	—
K	—	—	—	8.00

Formula of salt $\left. \begin{matrix} \text{K Ca}'' \\ \text{Cr}''' \end{matrix} \right\} 3\text{C}_2\text{O}_4 \cdot 4\text{H}_2\text{O}.$

		Found.		
	Calculated.	I.	II.	III.
Ca	= 8.56	8.48	8.52	8.53
K	= 8.35	8.00	—	—
Cr	= 11.13	11.15	10.57	10.63
H ₂ O	= 15.41	16.00	16.00	16.10

Solubility.—1 part of the salt dissolved in about 22 of water at 16° C., and in 5 parts at 100° C.

It may be noticed that the water found is a little above the calculated amount, just so much in fact as the chromium is too low; this is not to be wondered at when the singular nature of the salt is taken into account, a salt which at the ordinary atmospheric pressure and temperature loses 4 per cent., or 1 molecule of water in dry air—a substance that can be dried only by crushing between bibulous paper and exposure in an open place to cool air. The solution, of a greenish-purple colour, when boiled becomes more decidedly green, and assumes its original colour on cooling. A little calcium oxalate generally separates out during recrystallization, The salt is exceedingly stable, and it crystallizes with the greatest ease from hot or cold, acid or neutral solutions, and can be precipitated in the crystalline state. It is almost insoluble in strong hydrochloric acid.

The following are the properties of the crystals:—When deposited slowly they are formed in clusters, the individual crystals being never more than $\frac{1}{100}$ of an inch in thickness and $\frac{1}{2}$ an inch long. They possess a greenish-black lustre, and apparently consist of prisms with a rectangular base; they reflect white light. From hot solutions much smaller crystals deposit; their colour is olive-green in daylight, and

* Calculated from 0.424 grm. of platinum.

beetroot-red by gaslight. Under like circumstances the powdered substance is of the same colour. Minute crystals, examined in daylight by the microscope, are of a pale green tint; fragments of large crystals coarsely powdered appear of almost all tints, rich green and blue prevailing. Larger crystals, deposited by spontaneous evaporation of a cold solution, appear of two colours chiefly, a beautiful blue and rich green, with sometimes a tinge of deep red, their appearance being exceedingly charming.

Occasionally crystals of a good size, seen by reflected light, appear of two colours; this has been observed in a mass of crystals removed from a glass dish, where they had grown in contact with the glass; distinct blue and green patches of colour were seen intermingled. These crystals most likely had not true surfaces, but were covered with smaller crystals, so that light reflected from the large facets became colour by transmission through small crystals. A quantity of the salt crystallized in isolated prisms in the bottom of a beaker glass was examined by looking directly at the sun; thus viewed they were ruby-red; on turning away from the direct rays at an angle of 30° they appeared green; and on moving still further round, some of them appeared blue. We may thus vary the colour of the crystals by admitting white light to them at different angles. An experiment was made in order to ascertain whether viewing a crystal through different axes produced alterations in colour. It was not easy to see how this could be done, because a crystal $\frac{1}{16}$ of an inch thickness would certainly be too large to transmit light sufficiently well. I devised, however, the following plan, and successfully overcame the difficulty. Taking a quill tube of good glass free from streaks and bubbles, I drew this out sharply to a capillary size, and ran a minute crystal into it with a little of the mother-liquor; by blowing into the tube the liquid was slowly expelled, while the crystal was securely wedged into the fine end, and so remained stationary when the tube was turned over. To make a stage for holding this, a glass slip had a cork bored with a hole cemented to it; the tube being passed through the hole, could be turned over without displacement from the field of the microscope.

In the first observation the crystal looked opaque; but here and there an indistinct red light shone through on turning it; a green tint with a shade of red next made its appearance, the green became full and brilliant; opacity next succeeded, and then blue with a tinge of green; finally the full beautiful blue colour. In each revolution of a crystal there were two periods of blue, two of green, and two of opacity with a tinge of red. When a crystal so small as to be quite transparent in any position was examined in this manner, it was seen that purely red light was transmitted through its edges. It is thus rendered very evident that the play of colour is due to pleochroism, or the property which, by virtue of double refraction, some minerals possess of transmitting light of different tints through their different crystalline axes. I have examined many well-crystallized salts in the manner already described, and there are none which can compare with potassio-calcium chromic oxalate. The

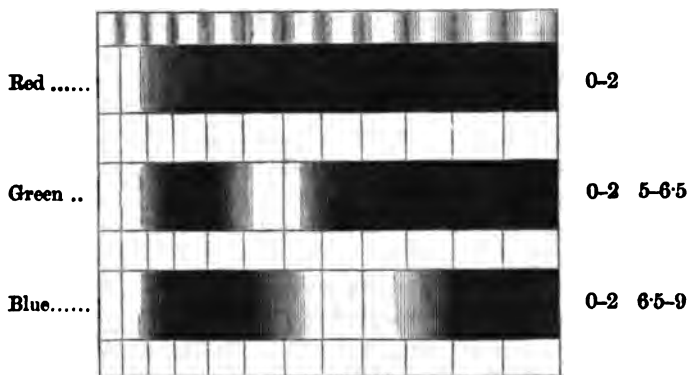
platinocyanides, most beautiful crystalline coloured bodies, attracted my attention; but their prominent properties are the converse of my salt; they reflect two or three different coloured rays, but transmit only one. Such salts are:—

Magnesium platinocyanide	Mg Cy ₂ . Pt Cy ₂ . 7H ₂ O.
Barium platinocyanide	Ba Cy ₂ . Pt Cy ₂ . 7H ₂ O,
Potassio-magnesium platinocyanide	Mg K ₂ . Cy ₄ . 2Pt Cy ₂ . 7H ₂ O.
Barium chloro-platinocyanide	5(PtCy ₄) Ba Pt Cy ₄ Cl ₂ . xH ₂ O.

The following I have also examined:—

Green salt of Magnus	2NH ₃ . Pt Cl ₂ ,
Roseo-cobaltic chloride	Co Cl ₂ . 5H ₂ N . H ₂ O,
and Claudet's salts	Co Cl ₂ . 5H ₂ N . Hg Cl ₂ .

Haidinger*, however, who examined the magnesium platinocyanide, found that although to the unaided eye the light transmitted was of a pure carmine-red, yet the dichroscopic lens showed that the colour of thin crystals was distinctly separated into crimson-red and a tint less blue. The two axes of *thin* crystals give spectra which do not differ from each other materially, nor from the solution of this salt in particular, nor from chromium salts generally. I noticed that solutions of the salt, when left to evaporate spontaneously in porcelain dishes, frequently formed crystalline crusts, which adhered to the sides for an inch or so above the liquid; and it occurred to me that such crusts would serve admirably for spectroscopic observations, isolated crystals being too small. Having purposely prepared a good quantity of the salt in this state, portions were selected, mounted, and examined with the micro-spectroscope in brilliant sunlight, with the result that three different spectra were observed. The accompanying sketch shows the relative positions of the transmitted rays.



The red tint, seen when looking at the diagonal to the base of the prism, consisted of one red luminous band only between 0 and 2 on

Sorby's scale. The green rays passing through a short axis consisted of the red band 0-2, and one green band between 5 and 6.5, the blue rays passing at right angles to the former being situated between 6.5 and 9. Very frequently the blue rays were associated with the red band 0-2. If these spectra be combined, the absorption-spectrum common to the solution and to thin crystals is produced.

Examined with a Nicol's prism and with similar crystals used as analyzers, it was found that this substance is a powerful polarizer. There is a marked difference when the blue and the green rays are examined with a Nicol's prism, the former being much more completely polarized than the latter, so that in rotating the Nicol there is an alternation of brilliant blue and perfect darkness—if the crystal be thin, of blue and reddish green. If, however, a crystal transmitting green rays of equal brilliancy be examined, the light is not cut off, but only obscured. In like manner, if both analyzer and polarizer consist of these crystals, when in a position at right angles to each other, the effect of course is the same as that produced by a Nicol's prism, viz. only green rays are transmitted. The green ray is more strongly refracted, and at the same time more absorbed than the blue, in accordance with Babinet's law (Pogg. '*Annalen*,' vol. xli. p. 116). This was made evident from their apparent change of colour when white light was admitted to them at different angles. These observations account for the facts already noticed—namely that minute crystals appear green even under the microscope, and also that the powder is green.

Let us take haphazard a number of crystals, or fragments of crystals, lying in all directions over each other, and it is easy to see that no blue rays can be transmitted; for where two crystals cross, the light transmitted is green. The green rays being more absorbed (that is to say, travelling with less facility) than the blue, it is possible to have a blue crystal so thin as to transmit white light through one axis, while green rays would pass at right angles through the other; under these circumstances the crystals would appear green. The red colour seen by gaslight is of course due to the suppression of the green and blue by the yellow illumination.

I find that Sir David Brewster*, and also Haidinger, have examined the blue potassium chromic oxalate of Gregory; and many of its properties are those also of my salt. Brewster† states:—"In the smaller crystals, which are generally the best formed, the colour of both reflected and transmitted daylight is blue, but that of candle-light is purplish." "The relations of this salt to plain and polarized light may be readily examined and finely exhibited by placing upon a plate of glass a few drops of a saturated solution of it in water. If the crystals are slowly formed, they will be found of various thicknesses, each thickness exhibiting a different colour, varying from perfect transparency through all shades of

* Phil. Trans. 1835, part 1.

† Phil. Mag. vol. vii. p. 435.

pale yellow, green, and blue in daylight, and through all shades of *pale yellow, pale orange, red, and blue* in candle-light." I find the colour of small crystals by reflected light is not always blue, but varies between blue and green, just as does the new salt under the circumstances already mentioned. Again, the variations in tint of small crystals is attributed to the differences in thickness; but it is really due to pleochroism of a character identical with that in my salt. A crystal powdered up coarsely, and placed under the microscope, no longer presents a blue colour, but consists of a beautiful collection of coloured fragments, decided *red, green, blue, and yellow*, besides all imaginable intermediate tints, such as neutral green and greenish blue. These tints change as the fragments roll over in the liquid surrounding them. This property is not noticed by Haidinger, who, in a contribution to Poggendorff's 'Annalen' * on the pleochroism of this potassium salt, states that the dichroscope showed the ordinary image to be green, and the extraordinary blue, the blue tint being the brighter, so that, the colours being brought together in daylight, the blue prevails.

Brewster noticed previously that, the images—produced by double refraction in these crystals, the least refracted was blue, and the most refracted green.

Bunsen† has obtained different absorption-spectra by passing polarized light through different axes of crystals of didymium sulphate.

Light in traversing a crystal of such a substance as this new compound is both coloured and polarized by the crystal itself; it is therefore impossible to regard its spectrum by other than polarized light. We therefore get different kinds of absorption in different crystalline axes, which are manifested in a striking manner by the varying colours the salt presents. This well exemplifies the fact that pleochroism results from the polarization of light traversing a doubly-refracting coloured medium.

From the microscopic examination of many similar new compounds, namely ammonio-calcium, potassio-ammonium, potassio-lithium, potassio-thallium, barium, and strontium chromic oxalates, it was found that the efficiency in displaying pleochroism was inversely as the molecular weight of the compounds, because the property depends upon the amount of colour the chromium gives to the crystals. The proportion of chromium is of course larger when it is associated with other elements possessing small atomic weights. And whether we have 40 parts of calcium or 406 of thallium in a compound associated with 52 of chromium, is a matter which greatly affects the percentage of chromium in the substance.

It was at first supposed, from the totally different appearance of the salt when precipitated, and when slowly formed in large crystals, that two compounds were obtained, it being a common occurrence to have

* Vol. lxxvi. p. 107.

† Pogg. Ann. vol. cxviii.

two modifications of a chromium salt, as for instance the blue and the green solutions of chromic nitrates and sulphates. Chemical analysis, however, showed them to be identical in composition, while the crystallization and close examination of many specimens disclosed the nature of the substance. I have come to the conclusion that this is certainly the most remarkable example of pleochroism at present known.

NOTE.—Received November 7, 1873.

I find that the potassio-calcium chromic oxalate is a substance which excels uranium compounds as a test for the purity of white light. It is well known that uranic salts exhibit dark bands in the blue part of the spectrum, which cannot be seen by gas-light although plainly visible by day; they are, however, shown with some distinctness by the lime-light. This chromium compound shows a red tint by gas-light, red and blue by the lime-light, while by the light of magnesium wire or the sun it displays red, blue, and green. So sensitive, indeed, is this substance that its true colour cannot be seen on an ordinary November morning in London, some crystals appearing blue, others of a colour varying between neutral grey, lilac, and red tints, according to the yellowness of the atmosphere.

To the list of substances examined for pleochroism, I have now to add, besides the minerals well known to possess this property:—

Sulphate of iodo-quinine.
Sulphate of iodo-cinchonidine.
Sulphate of iodo-quinidine.
Potassio-palladium chloride.
Ammonio-palladium chloride.
Potassium permanganate.
Teriodide of tetrethylammonium.

The powerful polarizing qualities of the first two iodine compounds are well known; they have been described by Dr. Herapath* and Prof. Stokes. As they crystallize, however, in thin plates, it is impossible to transmit light through more than one axis; they cannot therefore be brought into comparison with my substance. Endeavours to obtain large thick crystals did not meet with success. Sulphate of iodo-quinidine crystallizes in beautiful orange-brown prisms with a violet lustre; it is, however, but a feeble polarizer, and therefore does not exhibit pleochroism to any noticeable extent. The double palladium salts which have been previously examined are beautifully pleochroic prismatic crystals exhibiting an orange-brown tint along the principal crystalline axis, and a green in other positions. It is well seen in crystals $\frac{1}{2}$ millim. in thickness. Potassium permanganate is a substance which, seen even in minute crystals, is still opaque. A very dilute solution was allowed to

* Proc. Roy. Soc. viii. 1856–57. Phil. Mag. S. 4. vol. iii. 1852; iv. 1852; vi. 1853.

crystallize on a slip of glass, and crystals small enough to be transparent were examined by daylight with an $\frac{1}{8}$ -inch objective; no remarkable difference was observed in the colour of different crystals of the same thickness, and the polariscope did not show them to be pleochroic, the two contrasting colours, violet and red, being due to different thicknesses of crystals. To aid me in the above experiments, Messrs. Howards and Sons, of Stratford, very generously supplied me with the sulphates of cinchonidine and quinidine in a pure state; and I beg to offer them my best thanks for the kindness. The iodine compound of the tetraethylammonium occurs in prismatic crystals resembling the sulphate of iodoquinidine, but exhibits no property of interest.

II. "On the Quantitative Analysis of certain Alloys by means of the Spectroscope." By J. NORMAN LOCKYER, F.R.S., and WILLIAM CHANDLER ROBERTS, Chemist of the Mint. Received November 20, 1873.

(Abstract.)

The authors, after referring to experiments which showed clearly that the spectroscope might be employed to detect minute differences in the composition of certain alloys, proceed to give an account of the researches which they had instituted with a view to ascertain the degree of accuracy of which the method is capable.

The image of an electric spark passing between the unknown alloy and a fixed electrode being thrown by means of a lens on the slit of the spectroscope, the phenomena observed were found to vary with the composition of the alloys; and further, by arranging them together with known check-pieces on a suitable stand, and bringing them in turn under the fixed electrode, the composition of the unknown alloys was determined by comparison with the known check-pieces.

The shape of the electrode ultimately adopted was that represented in the sketch; and these pieces were held in their places by suitable metallic clips. Special attention was then directed to the adjustment of the length of the spark, which was found to materially influence the phenomena. The method adopted consisted in placing the variable electrode in the field of a fixed microscope having a 3- or 4-inch objective, and adjusting the summit of this electrode to coincide with the spider-lines of the eyepiece.



After a series of experiments on alloys of zinc and cadmium of various compositions, the results of which were shown on a curve, more extended trials were made with the gold-copper alloy employed in coinage, which was peculiarly suited to these researches in consequence of the known method of assay having been brought to so high a state of perfection (the composition being determined with accuracy to the $\frac{1}{10,000}$ part of the original assay-piece of about 7 grains), and from the fact that reliance can be

placed on its homogeneity. The paper is accompanied by a series of four curves, which show the results of experiments, and in which the coördinates are given by the ordinary method of assay, and by the spectroscopic readings.

The chief practical advantage which appeared to flow from this inquiry was that, if it were possible to replace the parting assay by the spectroscopical method, a great saving of time in ascertaining the value of gold bullion would be effected.

III. "Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun."—Part III. By J. NORMAN LOCKYER.
Received November 20, 1873.

(Abstract.)

The paper commences with an introduction, in which the general line of work since the last paper is indicated. Roughly speaking, this has been to ascertain the capabilities of the new method in a quantitative direction. It is stated that while qualitative spectrum-analysis depends upon the *positions* of the lines, quantitative spectrum-analysis on the other hand depends not on position but on the *length, brightness, and thickness* of the lines.

The necessity of maps carefully executed and showing the individuality of each line is shown; and it is stated that the execution of these maps required the use of the electric arc to render the vapours of the metals incandescent. A battery of 30 Grove's cells of one pint capacity was accordingly employed in the researches about to be described.

The difficulties of eye-observations of the characters of the lines compelled the application of photography, another reason for the use of which existed in the facility it afforded for confronting spectra with each other, and so eliminating coincident lines, since the lines, if due to impurities, would be longest and thickest in the spectrum to which they really belonged.

The portion of the spectrum at present worked upon is that from H to F.

Another branch of the research has been the construction of a Table of all the named Fraunhofer lines, showing the lengths and thicknesses of the metallic lines to the absorption of which they were due; this Table enabled the author to allocate upwards of 50 lines in the solar spectrum, presumably overlooked by Ångström and Thalén. The Table was intended as a preliminary to a new photographic map of the spectrum from H to F, on a larger scale than Ångström's, which was intended to clear away all the difficulties touching coincidences—and to have below it complete maps of all the solar elements with their long and short lines. This map is incomplete at present, but is making rapid progress.

A preliminary search for elements supposed not to be in the sun has also been commenced.

Of the above-named researches the subsequent parts of the paper refer to:—

- I. The experiments made on a possible quantitative spectrum-analysis.
- II. The method of photographing spectra adopted.
- III. The coincidences of spectrum lines.
- IV. The preliminary inquiry into the existence in the sun of elements not previously traced.

I. The experiments made on a possible quantitative Spectrum-Analysis.

After the two former papers were sent in to the Royal Society, an investigation of the general changes undergone by spectra given by alloys was commenced.

A micrometer eyepiece was mounted on the observing-telescope of the spectroscope. With this the following phenomena were observed:—

I. The lines which remained varied their length as the percentage of the elements to which they were due varied.

II. Some of the lines appreciably varied their thickness or brightness, or both, in the same way.

III. In cases where the brightness of a line was estimated through a considerable range of percentage composition by comparison with an air-line, the air-line was observed to grow faint and then disappear as the brightness of the metallic lines increased.

IV. In cases where the brightness or thickness of the line of one element was estimated by comparison with the line adjacent of the other constituent of the alloy, the point of equal brightness was observed to ascend or descend; this method was used to avoid the uncertainty of micrometric measurements of the tips of the lines in consequence of their variation in length due to the unequal action of the spark.

V. In some cases, where the percentage of a constituent was so small that none of its lines were visible, there yet seemed to be an effect produced on the vapour of the opposite pole.

As these conclusions were derived from coarse alloys, and it was desirable to observe the effect of very fine gradation, Mr. C. Fremantle, the Deputy Master of the Mint, was begged to allow observations to be made on the gold-copper and silver-copper coinage alloys; and he immediately responded most courteously to the request.

Examples of the behaviour of some coarse alloys of silver and lead are given; they were irregular in their action; but it was observed that silver lines remained in the alloy as long as from .05 to .02 per cent. of silver was present. The alloys, however, were very unequal. Experiments on cadmium and tin alloys are described, the cadmium forming 10, 5, 1.0, 0.15 per cent. In the last but one cadmium line was permanent; in the first at least five were seen. In an alloy of 0.099 per cent. of cadmium

with a mixture of lead, tin, and zinc constituting the rest of the alloy, the behaviour of the cadmium lines was sensibly the same as in a mixture of 0.1 per cent of cadmium and 99.9 of tin.

In the Mint specimens the same phenomena were observed *en petit* as the coarser alloys showed *en grand*. In a gold-copper alloy $\frac{1}{1000}$ increase in the gold made the lines shorter, and a similar increase in the copper made them longer.

In the silver-copper alloy an increase of $\frac{1}{1000}$ in the silver lengthened the lines, a similar increase in the copper shortened them.

These phenomena can be explained by assuming such alloys to be different physical things, and that the spark acts upon the alloy as a whole as well as upon each vapour separately.

Thus, in these Mint alloys, copper is common to both, and their melting-points are:—

Gold . . . 1200° (Pouillet).

Copper . . 1200° to 1000°, the precise point not determined.

Silver . . 1000° (Pouillet).

The intermediate position of copper explains the different action on its lines of gold and silver.

II. *The Method of photographing Spectra adopted.*

A camera carrying a 5 × 5-inch plate and a 3-inch lens of 23 inches focus, replaced the observing-telescope of the spectroscope. The lens focused from 3900 to 4500 very fairly upon the plate. The beam passing through collimator and prisms was, as in Mr. Rutherford's researches, very small. As the electric arc in its usual vertical position gave all the lines from pole to pole, the lamp was placed on its side, and the arc used in a horizontal position, the slit being vertical. The dense core of the arc then gave all the short lines in the centre of the field, the longer ones extending beyond them on either side. In order to obtain a scale, it was resolved to photograph the solar spectrum immediately adjacent to the metallic spectrum under examination.

To effect this a portion of the slit was covered up while the solar spectrum passed through the free part, and then the part used for the solar spectrum was covered, while the formerly covered part was opened for the metallic spectrum. This was effected by a shutter, with an opening sliding in front of the slit; a diagram of its action and form is given.

The arrangement of the spectroscope, heliostat, &c. for obtaining the sun's light is described. The image of the sun was brought to a focus between the poles of the lamp by an extra lens interposed between the lamp and the heliostat.

The use of the shutter enables us to compare either two or more spectra upon a single plate; or the solar spectrum may be compared with

two metallic spectra, being made to occupy the position between the two.

III. On the Lines coincident in different Spectra.

The bearing of the former papers on the lengths of the lines of the elements is briefly recapitulated.

The examination of the various spectra of metals and alloys indicated the great impurity of most of the metals used, and suggested the possibility of the coincidences observed by Thalén and others being explained in the light of former work.

It is observed that coincidences are particularly numerous in the spectra of iron, titanium, and calcium, and that nearly every other solar metallic spectrum has one or more lines coincident with lines of the last element. These coincident lines are, as a rule, very variable in length and intensity in various specimens of the metals in which they occur, and are sometimes altogether absent.

One of the longest calcium lines, that at wave-length 4226·3, is also seen in the strontium spectrum as a line of medium length; and 4607·5, a very long line in strontium, appears in calcium as a short line. Another very long strontium line, 4215·3, is asserted by Thalén to be seen in calcium; but the author has never seen it till lately, and then only in a specimen of calcium known to contain strontium.

We have here, then, a case of coincident lines, in which the one that is long and bright in one spectrum is short and faint in the other, and a case of a line said to be coincident in two spectra being, though always visible in one, sometimes absent in the other of them, and only appearing in it when the two substances were mixed. The hypothesis of impurity at once explains the whole case, even without the third line, which renders the fact of mixture certain.

The longest lines of calcium occur in iron, cobalt, nickel, barium, strontium, &c.; and the longest lines of iron occur in calcium, strontium, barium, and other metals.

Other cases are adduced; and the following general statements are hazarded, with a premise that further inquiry may modify them.

1. If the coincident lines of the metals be considered, those cases are rare in which the lines are of the first order of length in all the spectra to which they are common: those cases are much more frequent in which they are long in one spectrum and shorter in the others.

2. As a rule, in the instances of those lines of iron, cobalt, nickel, chromium, and manganese which are coincident with lines of calcium, the calcium lines are long, while the lines as they appear in the spectra of the other metals are shorter than the longest lines of those metals. Hence we are justified in assuming that short lines of iron, cobalt, nickel, chromium, and manganese, coincident with long and strong lines of calcium, are really due to traces of the latter metal occurring in the former as an impurity.

3. In cases of coincidences of lines found between various spectra the line may be fairly assumed to belong to that one in which it is longest and brightest.

A description of some photographs of spectra is then given, a photograph of the coincident lines of calcium and strontium being amongst them, and proving that strontium occurs in the sun; and the section concludes with a brief description of the method employed in making the new map, showing lengths and thicknesses, and enumerating coincident lines. This is done thus: papers are pasted on to photographs of the solar spectrum on glass; the lengths of the lines of the metallic spectrum under examination (*e.g.* that of iron) are marked on this paper in prolongation of the solar lines to which they correspond. They are then copied upon a map; and another piece of paper being fixed down, another spectrum is proceeded with in the same way.

IV. *The preliminary inquiry into the existence of elements in the Sun not previously traced.*

The previous researches having shown that the former test for the presence or absence of a metal in the sun, namely the presence or absence of its brightest or strongest lines in the average solar spectrum, was not conclusive, a preliminary search for other metals was determined on; and as a guide, Mr. R. J. Friswell was requested to prepare two lists, showing broadly the chief chemical characteristics of the elements traced and not traced in the sun.

The Tables showed that, in the main, those metals which had been traced formed stable compounds with oxygen.

The author therefore determined to search for the metals which formed strong oxides, but which had not yet been traced.

The result up to the present time has been that *strontium*, *cadmium*, *lead*, *cerium*, and *uranium* would seem with considerable probability to exist in the solar reversing layer. Should the presence of *cerium* and *uranium* be subsequently confirmed, the whole of the iron group of metals will thus have been found in the sun.

Certain metals forming unstable oxides, such as gold, silver, mercury, &c. were sought for and not found. The same was the case when chlorine, bromine, iodine, &c. were sought by means of their lines produced in tubes by the jar-spark. These elements are distinguishable as a group by forming compounds with hydrogen.

It is observed that certain elementary and compound gases effect their principal absorption on the most refrangible part of the spectrum when they are rare, and that as they become dense the absorption approaches the less-refrangible end—that the spectra of compounds are banded or columnar, the bands or columns lying at the red end of the spectrum—that the absorption spectra of chlorine, iodine, bromine, &c. are columnar, and that these are broken up by the spark just as the band

spectra of compounds are broken up—and that it is probable that no compounds exist in the sun. The following facts, gathered from the work already accomplished by Rutherford and Secchi, are stated.

There are three classes of stars :—

1. Those like Sirius, the brightest (and therefore hottest ?) star in the northern sky, their spectra showing only hydrogen lines very thick, and metallic lines exceedingly thin.

2. A class of stars with a spectrum differing only in degree from those of the class of Sirius ; and to this our sun belongs.

3. A class of stars with columnar or banded spectra indicating the formation of compounds.

The question is asked whether all the above facts cannot be grouped together in a working hypothesis, which assumes that in the reversing layers of the sun and stars various degrees of “celestial dissociation” are at work which prevents the coming together of the atoms which, at the temperature of the earth, and at all artificial temperatures yet attained here, form the metals, the metalloids, and compounds.

In other words, the metalloids are regarded as *quasi* compound bodies when in the state in which we know them ; and it is supposed that in the sun the temperature is too great to permit them to exist in that state in the reversing layer, though they may be found at the outer portions of the chromosphere or in the corona.

It is suggested that, if this hypothesis should gain strength from subsequent work, stony meteorites will represent the third class of metalloid or compound stars, and iron meteorites the other or metallic stars.

The paper concludes as follows :—

“An interesting physical speculation connected with this working hypothesis is the effect on the period of duration of a star's heat which would be brought about by assuming that the original atoms of which a star is composed are possessed of the increased potential energy of combination which this hypothesis endows them with. From the earliest phase of a star's life the dissipation of energy would, as it were, bring into play a new supply of heat, and so prolong the star's life.

“May it not also be, if chemists take up this question, which has arisen from the spectroscopic evidence of what I have before termed the plasticity of the molecules of the metalloids taken as a whole, that much of the power of variation which is at present accorded to metals may be traced home to the metalloids ? I need only refer to the fact that, so far as I can learn, all so-called changes of atomicity take place when metalloids are involved, and not when the metals alone are in question.

“As instances of these, I may refer to the triatomic combinations formed with chlorine, oxygen, sulphur, &c. in the case of tetrad or hexad metals. May not this be explained by the plasticity of the metalloids in question ?

“May we not from these ideas be justified in defining a metal, provi-

sionally, as a substance the absorption-spectrum of which is generally the same as the radiation spectrum, while the metalloids are substances the absorption-spectrum of which, generally, is not the same?

"In other words, in passing from a hot to a comparatively cold state, the plasticity of these latter comes into play, and we get a new molecular arrangement. Hence are we not justified in asking whether the change from oxygen to ozone is but a type of what takes place in all metalloids?"

Presents received November 20, 1873.

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END OF THE TWENTY-FIRST VOLUME.

PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

JOHN BISHOP was born on the 15th September, 1797, and died on the 29th September, 1873. He was the fourth son of Samuel Bishop, Esq., of Pimperne, in the county of Dorset, and received his education at the Grammar School of Mr. Longman at Childe Okeford in the same county, where he remained several years. He was originally intended for the legal profession; but negotiations for this object having been broken off through some misunderstanding between the contracting parties, he pursued for some time the idle but agreeable life of a country gentleman, and his favourite field sports, hunting and shooting. When about five and twenty years of age, he was induced by his cousin, Mr. John Tucker, a surgeon of Bridport, to enter the medical profession; after a short preliminary study and practice at Bridport, he came to London, where he entered at St. George's Hospital under Sir Everard Home. During his student days he attended the lectures of Sir Charles Bell, then in his zenith, of Mr. Guthrie, Dr. George Pearson, and the Chemical Courses at the Royal Institution. In 1824 he obtained the diploma of the Royal College of Surgeons, and in 1851 was elected a Member of the Council of the College, where he retained a seat for nearly twelve years. In 1844, having previously published a paper in the *Philosophical Transactions* on the Physiology of the Human Voice, he was elected a Fellow of the Royal Society, and a Corresponding Member of the Medical Societies of Berlin and Madrid. About this time he also obtained from the Royal Academy of Sciences, Paris, two prizes for memoirs on the Human and Comparative Anatomy and Physiology of Voice. He contributed to Todd's 'Cyclopædia' the articles "Larynx," "Motion," and "Voice," and was the author of works on Distortions of the Human Body, on Impediments of Speech, and on Hearing and Speaking Instruments. These works, together with several minor contributions to medical literature, gained for Mr. Bishop the well-deserved reputation of a careful and skilful observer, and are remarkable for the evident pains taken by the author to examine and do justice to whatever had been written by others upon the subject he had under consideration, and, where practicable, to prove by mathematical demonstration every theory he himself advanced.

During his career Mr. Bishop held the offices of Senior Surgeon to the Islington Dispensary, Surgeon to the Northern Dispensary, to the St. Pancras Dispensary, and to the Drapers' Benevolent Institution. In 1852 he was elected President of the Medical Society of London, and held the offices of Trustee, Councillor, Orator, and Lettsomian Lecturer to the Society. He was also for many years, and at the time of his death, one of the managers of the Russell Institution. He was a man of considerable energy of character; and although undemonstrative in his manner, entered zealously into any business he undertook, and exercised great personal influence upon his colleagues. His judgments, deliberately formed and carefully expressed, carried considerable weight with them, and influenced

the decision of many a council meeting. He had the tact of opposing without irritating; and even those to whom on questions of public business he might find himself opposed, were ever ready to do justice to the honesty of purpose and singleness of aim by which he was actuated. Of many and varied attainments, an enthusiastic admirer of music and of art, conversant with continental as with English literature, he continued until within a few months of his death to take an active interest in all the discoveries and achievements of modern science. The philosophical bias given to his mind by the eloquent teachings of Sir Charles Bell remained with him to the last, and was manifested both in his writings and in his conversation.

Full of years, tended by those he loved, conscious of having gained and maintained to the last an honourable if not distinguished reputation, at Strangeways-Marshall, Dorset, within a few miles of his birthplace, this estimable life drew to a close. It is no exaggeration to say that by all who were acquainted with him Mr. Bishop was respected and esteemed; but only those who knew him intimately can bear full testimony to the kindly feeling and genial worth that preeminently characterized the subject of this imperfect memoir.

BALDWIN FRANCIS DUPPA was the eldest son of Baldwin Francis Duppa, B.A. (Oxon.), and Barrister-at-law. His parents spent the first few years of their married life abroad; and their eldest son and first child was born, February 18, 1828, at Rouen in Normandy. His early taste for experimental investigation involved young Duppa in a most disastrous accident; for when he was only seven years old he poured the contents of a powder-flask into the fire, causing an explosion which shattered his hand, blinded him for some time, and deprived him of the use of a thumb for life.

He received his early education at the celebrated school of M. de Fellenberg at Hofwyl near Berne, where he was the intimate companion of Henri the grandson of De Saussure. Here he acquired a good knowledge of French, German, and Swiss-German, and spoke the first two languages at that time so well that in returning from Switzerland he was twice taken for a runaway from school (once for a French boy and once for a German), and had some trouble to persuade the diligence officials to let him pass. To these languages he subsequently added Italian and Arabic. It was one of the features at Hofwyl that each boy should learn some useful trade during play hours; and young Duppa took up carpentering, becoming very expert in the making of ornamental boxes and other small articles. To this early development of mechanical skill was doubtless due some of that extraordinary manipulative power and fertility of invention which afterwards characterized his laboratory work. Quickly seizing upon the essential conditions for the performance of a given chemical operation, the prompt construction of the necessary apparatus,

out of any materials which happened to be at hand, seemed to cost him no trouble : glass, wood, and metal were all alike made to obey his hand ; and the suggestion of an experiment, chemical or physical, had not usually to wait many hours for its realization.

Duppa retained to the last a great affection for this school, often speaking of the happiness of the life he led there and of the excellency of the training, in the highest terms, and contrasting it with an English school to which he was afterwards sent, where he was very unhappy and learnt nothing. At Hofwyl there were no rewards and no punishments strictly so called, and the pupils were encouraged to cultivate habits of observation.

When only twelve years old he lost his father, who died at the early age of thirty-seven. He was now removed from the Swiss school after spending only three years there ; nevertheless he always contended that he received his education at Hofwyl. After leaving the English school he had some private tuition, and then proceeded to Cambridge, where he made the acquaintance of Prof. W. Hallowes Miller, for whom he ever afterwards retained a warm regard. He left Cambridge without taking a degree, and began to read for the bar ; but finding the sedentary work utterly repugnant to his tastes, he abandoned it and went to Italy for three years to recruit his health, which was, even at this time, by no means strong. Here he turned to account the knowledge of mineralogy which he had acquired at Cambridge, and made a small collection of rare and interesting minerals. Returning from Italy in the year 1854, he determined to devote himself to the study of Chemistry. With this view he entered the Royal College of Chemistry as a pupil of Dr. Hofmann in the autumn of 1855. We find him in 1857 engaged upon his first original investigation on the action of sulphuric acid upon salicylic acid, by which he obtained sulphosalicylic acid*. He afterwards prepared and investigated bromide of titanium. On leaving the College of Chemistry he fitted up a laboratory in his country seat at Hollingbourne near Maidstone, where, in conjunction with Mr. W. H. Perkin, he commenced a series of most interesting and important investigations on the substitution products of acetic, malic, and succinic acids, which culminated in one of the greatest achievements of synthetical chemistry—the artificial production of tartaric acid. These researches are described in seven memoirs published in the *Philosophical Magazine* and the *Journal of the Chemical Society*†.

Finding his residence at Hollingbourne severed him almost entirely from scientific associates, he abandoned his private laboratory, came in 1860 to reside in London, and worked for some months in the laboratory of Dr. Frankland at St. Bartholomew's Hospital. Here he

* *Jahresbericht der Chemie*, 1857, p. 322.

† *Phil. Mag.* (4) xiv. 217 ; *Quarterly Journal of Chem. Soc.* xi. 22 ; *Phil. Mag.* (4) xvii. 280 ; *Phil. Mag.* (4) xviii. 54 ; *Quarterly Journ. of Chem. Soc.* xiii. 1, 2, and 102.

devoted himself in the first instance to a study of the modern delicate methods of gas-analysis. Over these his great manipulative skill soon gave him the complete mastery. At that time Frankland was engaged in the investigation of the action of zincethyl upon boric ethylate; and in this research Duppa eagerly joined. The first results were recorded in a joint paper communicated to the Royal Society in July 1860*. In this paper, the discovery of boric ethide, boric etho-diethylate, and boric etho-dihydrate was announced, and the chief properties of these bodies described. The materials for this paper had scarcely been obtained, when Duppa's health became so seriously impaired that he was obliged to leave England and spend a couple of years in Italy and Algeria, whence he returned in 1863 and rejoined Frankland, who had, in the mean time, been translated to the laboratory of the Royal Institution. Here, in the quiet of a gloomy room in the basement, the two chemists, free from the wearing duties of teaching the elements of the science, devoted themselves heart and soul to original research; the inventive and constructive skill of Duppa, aided by the liberality of the Managers of the Institution, soon transformed this gloomy cell into a laboratory which might be fairly regarded as a model of convenience for the class of work carried on in it. At first the neighbours were loud in their complaints of bad odours; but it was discovered that these could be obviated by securely closing the windows of the laboratory, which was done. The atmosphere inside was thus rendered anything but agreeable; it was, indeed, sometimes so highly charged with vapours as almost to prevent the eye from penetrating from one side of the room to the other: but this was not found to be an unmitigated evil; it effectually prevented those interruptions to work which are so common in London; for no non-worker could bear in the room more than a few minutes, and rarely visited it a second time. One visitor nearly fainted, and had to be quickly removed to fresh air to revive him. Thanks to the Royal Institution Research Fund, and to the Government Grant administered by the Royal Society, the means for the purchase of apparatus and chemicals were never wanting.

From this laboratory there issued in three years no less than fourteen papers, of which Duppa was the joint author, descriptive of original researches, and published in the *Philosophical Transactions*, the *Proceedings of the Royal Society*, and the *Chemical Society's Journal*.

These researches related, first, to the synthetical genesis of acids of the lactic, acetic, and acrylic series; secondly, to the production of compounds of mercury with the organic radicals methyl, ethyl, and amyl; thirdly, to the synthetical formation of ketones; and fourthly, to the transformation of organo-mercury compounds into organo-zinc compounds. The records of these researches constitute the best testimony

* *Proc. Roy. Soc.* vol. x. p. 568.

to the enthusiastic zeal with which Mr. Duppa prosecuted chemical investigation during these years.

In 1867 Dr. Frankland resigned his professorship at the Royal Institution; and Mr. Duppa accompanied him to the private laboratory at the Government College of Chemistry, which was then in its old quarters in Oxford Street; but the meagre resources of this laboratory could not, even in his hands, be turned to account for the prosecution of these investigations, and the work had to be abandoned in despair; or rather it was postponed in anticipation of the completion of the new laboratories at South Kensington, which had been promised from year to year. At length, early in the present year, the new research laboratory was partially completed, and Duppa, all eagerness to recommence the work so long in abeyance, took a house in London, solely to enable him to prosecute his chemical labours. Alas! it was too late. Before he could again resume that which always afforded him so much pure pleasure, he was attacked by the malady which prematurely cut short his useful life. He died of consumption at Budleigh-Salterton, Devonshire, on the 10th of November, aged forty-five years, retaining his interest in experimental science to the last. A few hours before his death he dictated a long letter, accompanied by a diagram, to Professor Tyndall on a curious optical phenomenon which he had recently observed.

Mr. Duppa was elected a Fellow of the Royal Society in 1867. He married in 1869 Adeline Frances Mary Dart, the only surviving child of J. H. Dart, Esq., Barrister-at-Law, of Beech House near Ringwood, Hants. He leaves a widow and one child. He was a warm-hearted and faithful friend, and, as a landowner and Justice of the Peace in Kent, he was well known and much esteemed. His devotion to science did not prevent his taking a deep interest in art. He was a painter of no inconsiderable ability; and from his frequent wanderings in search of health in France, Spain, Switzerland, Italy, Algeria, and Madeira he always brought home his portfolio well filled with beautiful and interesting sketches, in which he sometimes embodied the effects of ancient and modern volcanic and glacial action with great vividness and truth.

Having a small private fortune, he never applied for nor held any scientific appointment. He always entertained a disinclination to lecturing and public speaking. Had this been otherwise, he could scarcely have failed to make his mark as a lecturer on his favourite science; for in conversation he had few rivals in popular scientific exposition, and in the laboratory perhaps none in devising appropriate experimental illustrations of chemical phenomena. He was one of that small band of enthusiastic and disinterested amateur labourers who have performed so large a proportion of the original scientific work of this country, and of whom England may justly feel proud.

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